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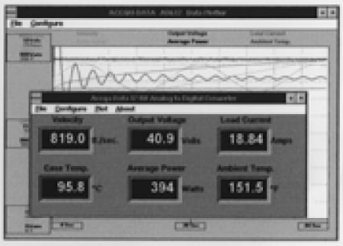

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# Infinite Energy

## Cold Fusion and New Energy Technology

**Volume 1, No.3, July/August 1995**

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**COVER:**
Night-time thermonuclear explosion, "Pamlico Event," July 11, 1962, near Christmas Island, U.S. "Operation Dominic" Series. Low-yield air-drop bomb test, before 1963 atmospheric nuclear test ban. *Photo: Courtesy, Lawrence Livermore National Laboratory*

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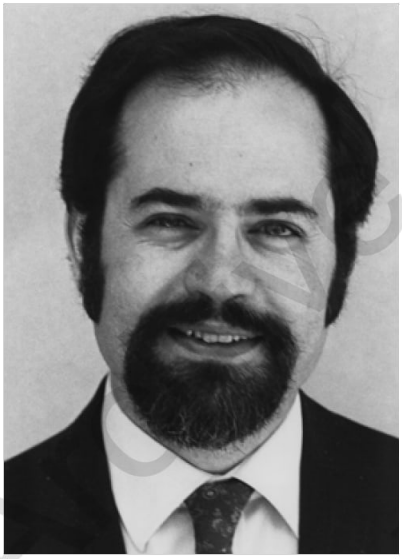
By Eugene F. Mallove, Sc.D.



# The Tip of an Iceberg

**When Drs. Martin Fleischmann and Stanley Pons made their seminal announcement** at the University of Utah on March 23, 1989, no one could have predicted that they were truly heralding the end of the Fossil Fuel Age.

The claimed nuclear reactions at room temperature and nuclear-level "excess power" in an electrochemical cell—much more thermal power out than electric power in — was just too much to swallow on Day One. On the other hand, there was a little-noticed remarkable coincidence, a profile of the future in its synchronicity: the announcement of cold fusion came less than twelve hours before the Exxon Valdez ran aground off the coast of Alaska and spilled millions of gallons of crude oil into pristine waters. It was the ultimate symbol of the bankruptcy—and impending demise— of reliance on hydrocarbon fuels to power technological civilization.

It took many months for the reality of the astonishingly powerful and mysterious energy source within water to be confirmed by a host of other laboratories. Now, there are disbelievers only among the under-informed, those who accepted the anti-scientific propaganda of cold fusion's opponents, and members of the Flat Earth Society—the professional "skeptics" who actively wage war against cold fusion. The latter are devout Believers that there can be nothing much new under the Sun in physics, save for what is supposedly allowed by Physics' venerable "sacred texts."

The last six years have been intensely frustrating, as the predicted agonies of a Kuhnian paradigm shift in science have been visited upon first hundreds, then thousands of cold fusion researchers and supporters world-wide. To be sure, enormous progress has been made in sharpening experiments and persuading other scientists and technologists to put aside their prejudices, but *the goal posts keep getting shifted.*

It now transpires that *no experiment*—no matter how iron-clad and provocative— ap-pears to be good enough to move the external debate off square one. The peer-reviewed published literature provides overwhelming support for both the nuclear-scale excess heat and nuclear changes in what were supposed to be exclusively *chemically* active systems. But this has not moved the reigning scientific establishment and its sheep-like acolytes in what passes today for "science journalism" in this area. That is why, for example, one has not read about cold fusion in the highly influential *New York Times* since 1992—except in occasional jokes or ill-conceived comparisons. It should be abundantly clear to the cold fusion community that only working, commercial devices capable of such feats as heating homes or running cars will force the an arrogant Science Establishment to eat crow—and perhaps expire from indigestion!

Far more important than the media morass and political forces arrayed against this new science is the amazing new scientific reality that is inexorably emerging. It is an irony of scientific history that the monumental heresy of Drs. Pons and Fleischmann in 1989—which would have been heresy enough!—was truly but the *tip of an iceberg* of the much greater scientific heresy lurking below. Signs of this emerged in the spring of 1991, when Dr. Randall Mills and colleagues in Lancaster, Pennsylvania announced that *ordinary* water could manifest the excess energy phenomenon with nickel electrodes—instead of the heavy water and palladium of the Fleischmann-Pons effect.

Soon thereafter came reports of transmutations of heavy elements in various cold fusion experiments—both in ordinary water and in heavy water systems—potassium changed to calcium, rubidium to strontium, and palladium to silver, rhodium, ruthenium, etc., etc. This alchemical "magic" complemented the roster of dozens of laboratories that had already detected the production of radioactive tritium in cold fusion, and the handful of labs that had reported solid evidence of the generation of helium-4—the "Holy Grail" of cold fusion that was angrily demanded as essential evidence by the critics. See, the Moving Goal Posts phenomenon at work! In this issue of *IE* you can read Hal Fox's report about a conference on low-energy nuclear transmutations, which was held on June 19 at Texas A&M University. Proceedings of the conference will soon be published, but already many of these experiments and theories have appeared in the scientific literature. We have no doubt that not all of these transmutation reports are of equal validity. Some may be on shaky experimental ground, but certainly not all of them! After all, a critic of cold fusion, Dr. Kevin Wolf, accidentally obtained some of the best evidence to date of low-energy transmutations (see *IE*, issue #2, May/June, 1995).

Where will it all end? The answer is that the bottom of the iceberg has not yet been reached and may be far, far below the surface. We are going deeper and deeper into cold water and finding that there is no end yet in sight to that mountain of ice—the tip of which figuratively sunk the Exxon Valdez and all that it represented. It is now clear that cavitation phenomena are capable of releasing energy from water—and perhaps from other fluids. The Hydrosonic Pump™ of Hydro Dynamics, Inc. is the paramount example of this, because that device has been rigorously tested and found to be producing substantial excess power—in the kilowatt range.

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In the last issue of *Infinite Energy* we told of Dr. Potapov's commercial success in eastern Europe with his series of Thermogenerator water cavitation heaters, being sold by his Vizor Corporation of Moldova. During the past few months, testing of the heart of the Potapov device —its vortex chamber and cavitation tube— at two sites in the U.S., failed to demonstrate any excess power, although it is still possible, perhaps likely, that not all proper conditions of operation were adhered to. That is very regrettable, since the claimed performance of these thermogenerators is at high multiples of input power. But the Moldova story continues. As this issue went to press, contributing editor Chris Tinsley was on his way to St. Petersburg, Russia, to participate with others in the testing of a full-system Yusmar-2 device in "as shipped" condition. This should give the Yusmar the best chance to prove itself, but we were certainly disheartened not to find what we had hoped to see this summer in long and exhausting verification experiments.

In the meantime, we present the results of the tests at the two U.S sites, as well as tests done in the Republic of Belarus, which were nominally positive, plus several other reports. Continuing the cavitation saga, we offer condensations of various older test reports of the Schaeffer cavitation heater, which was the precursor to the Hydro Dynamics, Inc. device. These test reports, provided to *IE* by Michael Huffman, are highly suggestive of excess power in the Schaeffer machine. Michael Huffman's device, reported by him in *IE* issue #1, is also likely to evidence the excess energy phenomenon, in his accounting of its testing. Now he holds a U.S. patent, #5,419,306, on his device.

It could be that the answer to a lot of what is going on in this field will be found "in the bubbles." The strange phenomenon of sonoluminescence, which physics Nobel laureate Julian Schwinger had metaphorically linked with cold fusion, is getting increased attention in the scientific literature and media. We recommend reading "Bubbles Turn Sound Into Light," by Seth J. Putterman, *Scientific American*, Vol.272, February 1995, pp.46-51. Our understanding of sonoluminescence is still in its infancy, but it is a fair assumption that sonoluminescence will play a role in defining the outlines of the new energy iceberg.

The foregoing heresies aside, perhaps *nothing* could be more antithetical to accepted physics than an electrical device with magnets and coils that would (over a sustained period) put out more energy than input to it. Do such machines exist? We are not sure, though this editor is of the opinion that they might well exist. The stunning Japanese FUJI television doumentary of October 1993, (see IE issue #1) on its face suggests that respectable scientists and companies in Japan have really created an *unthinkable* over-unity magnetic motor technology. And Bertil Werjefelt of PolyTech(USA) in Hawaii, a highly skilled engineer and industrialist, insists that he has observed these over-unity effects in his own experiments. At present he is expending large resources to make a powerful demonstration unit.

Now comes the astonishing peer-reviewed technical paper by Osamu Ide in the American Institute of Physics' *Journal of Applied Physics* (Vol.77, 1 June 1995, pp.6015-6020). This article, "Increased voltage phenomenon in


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a resonance circuit of unconventional magnetic configuration," reports on what appears to be a genuine electromagnetic anomaly in an electrical motor. Though the article is understated, if the data are correct, there appears to be a strong suggestion of a mechanism that could produce an over-unity electrical motor. Something in that motor is not working according to accepted physics. Energy is being recovered that shouldn't be! We find it enticing that Osamu Ide is affiliated with the Clean Energy Laboratory, Natural Group Corporation, in Shinegawa, Japan. Could this article in *AJP* be the tip of the iceberg of "hyper-efficient" electrical motor technologies in Japan? We shall see.

Could all these heresies have some common physical basis? Is there a fundamental flaw or flaws in physics— a "crack" or cracks in its foundations—that allow these things to occur? Most cold fusion people think not. They accept what might be called the "central dogma" of "mainline" cold fusion: nuclear ash commensurate with the excess energy production *will be* identified. They deny the possibility that something akin to tapping of Zero Point Energy (ZPE)—the microcosmic quantum fluctuations of space-time—might be the energy source at the bottom of the pyramid (iceberg?) and might even be helping to cause some of the observed nuclear reactions. There are now peer-reviewed technical papers that suggest this could be so, e.g. "Extracting energy and heat from the vacuum," by Daniel C. Cole (IBM Corporation) and Harold E. Puthoff (Institute for Advanced Studies at Austin), *Physical Review E*, August 1993, Volume 48, Number 2, pp.1562-1565.

We are satisfied to let the truth about the new source—or *sources*— of energy fall where it may. We are not going to argue with Mother Nature—that's futile. Experiments are primary; theory must take a back seat. It is appropriate to be guided by established theory—just don't get blinded by it. And let us enjoy our trip to the bottom of the iceberg—*if* there is a bottom!

A word about the cover of this issue, which comes near the 50th anniversary of end of World War II. The thermonuclear explosion in the night sky of July 11, 1962 (the Pamlico event) near Christmas Island in the Pacific Ocean was a "low-yield" U.S. bomb test, air-dropped, part of the "Operation Dominic" series.

Though some might argue the point, we do not consider the message on the cover to be hyperbolic. This thermonuclear power held the world on the brink of Apocalypse for the decades of the Cold War, while scientists in hot fusion laboratories around the world struggled to tame that same fusion energy for the benefit of humankind. Their long effort is now nearing an end. With massive funding cutbacks, the once noble tokamak hot fusion quest is now dying a much-deserved death in the United States; soon it will die elsewhere. Many of the hot fusioneers, feeling threatened, tried to kill cold fusion. They failed. They were the front-line shock troops with bags of dirty tricks, which caused a disgraceful hiatus in cold fusion development. The job of ending the old order—abolishing fossil fuel combustion globally as soon as possible—now falls to those who are open to the new paradigms of physics and chemistry.


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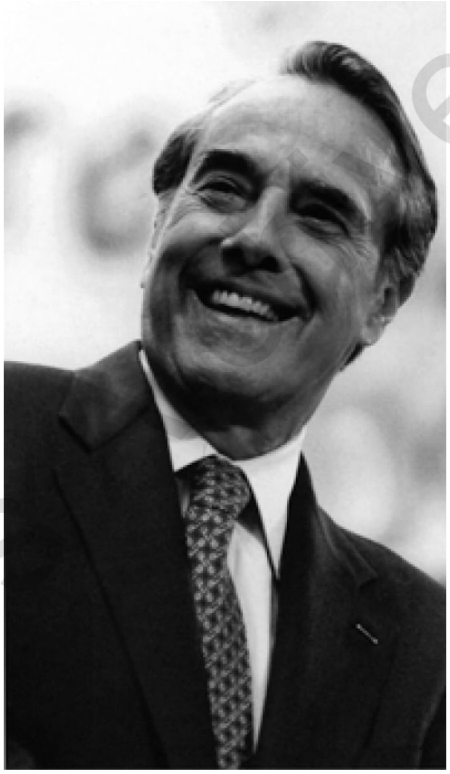

U.S. Senator Bob Dole (Republican of Kansas), running for President.

August 8, 1995

Dear Dr. Mallove,

Congratulations on the publication of your new magazine *Infinite Energy*. New developments in the area of cold fusion technology are constantly taking place and it seems that your magazine is on the leading edge of those developments.

Please also send my thanks to the anonymous contributor of my subscription. It will be a valuable addition to our energy research resources.

Again, my thanks.

Sincerely,
(hand-signed)
**BOB DOLE**
United States Senate

---

Infinite Energy #1 was great. I've

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been wanting to write but of course have always been tied up or working on my ZPE experiments. The two favorites are Michael Huffman's "From a Sea of Water to a Sea of energy" and Tom Benson's "A Microfusion reactor." *Infinite Energy* #2was even better than #1. "Chris Tinsley's Flying Circus"covering the Moldova cavitation heater was a joy to read. Not only are we told about a commercial (cold-fusion/ZPE) success story but within is some humor that we all need. This energy thing is serious business; however, a story like Chris's is a breath of fresh air.

Thanks for producing a fine magazine.

Bruce Vicknair
Baytown, Texas

---

Congratulations on the success of *Infinite Energy*. We have received an overwhelming positive repsonse from your readers. I will keep you abreast of future developments.

James W. Reding
President and CEO
Clean Energy Technologies, Inc.

---

The meeting of June 19th here went (in the opinion of many) rather well. At least 9 papers describing transmutation in the cold were presented. Frankly, I was myself amazed. Of course, a certain professor here and his associates claimed the meeting was a hoax. I undertook not to stimulate publication in order to minimize destructive press portrayals ("alchemy"!) The July 95 issue of *New Energy News* [by *IE* Associate Editor, Hal Fox - Ed.] has a good write up.

I admired your recent piece [IE #2 on Wolf!]

J. O'M. Bockris
Distinguished Professor of Chemistry
Texas A&M University

---

**An Open Letter from a Science Fiction Writer**

Dear Cold Fusion Guys,

What are you trying to do to me--put me out of business? I write science fiction, and you're not helping me by *doing* science fiction. And don't you know that the hoary old chestnut about the tinkerer in the back yard coming up with some fantastic invention that saves the world went out in the 1930s, along with homemade spaceships to the Moon?

Enough already.

I wouldn't mind so much if you guys had a nice, consistent theory that I could work from to make projections about the future. But all I keep hearing about are these contradictory results from experiments that all yield extra energy. C'mon now! It wasn't so bad when you were claiming cold fusion with palladium electrodes and heavy water--all I'd have to do in that case is stick one of those "Mr. Fusion" things from Back to the Future in every appliance or conveyance and I'd be off to the races.

But now you're talking about other stuff, weird stuff, and I don't know how to handle it. What's with this cavitation business anyway? Water bubbles? Is the future to be Jacuzzi powered? I'd be laughed out of business if I wrote a story about that. And energy from crystals? Crystals! That's for fantasy writers. I don't care if its piezoelectric or what—they're still just damn crystals.

Look, can you do me and the other SF writers a favor and just settle on some kind of explanation for over-unity phenomena—all of them, please—that we can rely on for a few years? You see, it usually takes six to eight months from the time a story is sold to the time it sees print, and we'd hate to have our science become outdated before a story hits the magazine rack. (I like the idea about zero point energy—I can baffle 99 % of my readers with that one.)

Anyway, thanks for the listen. But hurry up.

Jeffery D. Kooistra
Kentwood, Michigan

*Thanks, Jeff, we understand your impatience. We're working round-the-clock toward an answer! Our good friend Arthur C. Clarke says he agrees with you. —Gene Mallove*

---

I first heard of your magazine through Mark Goldes of Magnetic Power Incorporated in Sebastopol, CA. I enjoyed it immensely. You are doing a great service by keeping the faith in CF technology and other energy sources on the cutting edge. Keep up the good work.

Alan Berick, Ph.D.
Cottage Grove, Oregon

---

I am a student of Electrical Engineering and Computer Sciences (EECS) at the University of California at Berkeley, and I have recently discovered the Cold Fusion Technology home page on the World Wide Web. I am incredibly fascinated by the possibilities of Cold Fusion. I was only twelve years old when Doctors Pons and Fleischmann announced their findings back in 1989, and I remember my Dad explaining the implications of their results. When it came out that the results were [allegedly] false, my Dad made a comment that he believed that they were on to something because the Japanese hired them. Since I recently acquired access to the WWW, I decided to see if there was any information about Cold Fusion, and I was overwhelmed by what I found.

I'm just starting my sophomore year of undergraduate work, and I'm going through some soul searching as to what I want to do when I graduate. Since I am so interested in Cold Fusion, I was wondering if you had any advice on whether or not I should try to get a career in that field. If applications of Cold Fusion are as close to marketing as they sound, will there not be a big demand for people in majors such as mine? Are there companies that are working on Cold Fusion that are offering internships? I would greatly appreciate any feedback you could give me in this area.

T. Volkert

---

I was so excited to see the information about Cold Fusion on the Web. When Drs. Pons and Fleischmann made their announcement in the late 1980s, I tried to persuade some of the physicists here at Colby College to try the experiment, but I was essentially laughed out of the department. Since then, it has been hard to follow the progress of cold fusion as the media and "established" science community regard it as a hoax.

What a depressing experience I had in 1989! I was an undergrad independent major splitting my time between the physics and geo departments when the Utah breakthrough was announced. I wanted to try to replicate the experiment but I could not get any of the professors to give it any serious consideration.

We had a visiting chemist from Texas A&M who told me that they were doing similar experiments there with some similar results, but he would not support me in public as he was untenured.

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It was my first real experience with how "convention" can strangle science!

Its a wonder to me that we have advanced as far as we have in the face of such opposition in all fields of science. I tried to follow the Utah work until Pons and Fleischmann left the country and the whole concept was "debunked" by MIT and CalTech. I did catch the *Popular Science* article a few years back [Aug. 93], but the media is pretty much ignoring the whole subject of benchtop fusion.

I fooled around with the Pons device in my basement for a while but its not easy to get deuterium and palladium if you are not connected to the normal laboratory channels. I tried to make my own deuterium by using a method described in the encyclopedia which uses the slight differences in boiling temperatures between normal and heavy water to sort the molecules, but I had no way to test my finished product for purity. I am really excited about the work with nickel in normal water being done now.

I got my degree from Colby in Science & Technology Studies (their name for a general physical science degree) and came back to work in the Administration. Yesterday I stumbled on your home page on the web and was up late reading all about recent advances.

You must feel like Fermi, Bohr and Einstein felt fifty years ago. What an incredible place to be, on the leading edge of a technology that could revolutionize society! I look forward to following the work of you and others and I will remember back to this time when I switch on my cold fusion furnace a decade from now!

Ben Jorgensen
Director, Student Activities & Union Operations Colby College
Waterville, Maine

*"All progress has resulted from people who took unpopular positions" — Adlai Stevenson*

Ben, make that five years from now – or less. —*The Editor*

---

Where does Tinsley come up with "Water Fuel Device," for this outstanding "Yusmar" system?!! I've seen better and more accurate *titles* in various supermarket rags!

Where did I get the silly idea that electricity is the "fuel" that runs the motor that turns the vortex turbine, that produces the extremely high water temperatures within the water bubble vortexes?

Also, while the article is well done in structure and content, there is a *glaring* omission of the work of Viktor Schauberger and his very extensive work in hydrology, and introduction of the vortex turbine. Some of us believe that Schauberger has provided the whole antecedent basis for this present breakthrough. Omitting Schauberger from due credit is almost like omitting Tesla from the invention of the sychronous AC induction motor, in my view! It would proabably be worthwhile for you to do an historical follow up on the Yusmar concept—as soon as possible. ...On another topic, it now looks as if Hal Puthoff was correct in his evaluation of the MRA Units, [see *IE* #2-Ed.] which does not help Wootan and McClain very much. But we owe Wootan and McClain much applause for showing us how we can use transformer winding over Ba-Fe permanent magnets, which has never been done before. I suspect that this may be their major contribution to the new energy field.

Don A. Kelly, S.E.A.
Clearwater, Florida


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# DOES LOW-TEMPERATURE NUCLEAR CHANGE OCCUR IN SOLIDS? *A Report on the Low-Energy Transmutation Conference held at Texas A&M University, June 19, 1995*

by Hal Fox

(Reprinted, with permission from *Fusion Facts*, July 1995.

**INTRODUCTION BY DR. BOCKRIS**

Dr. John O'M. Bockris sponsored this historic conference and welcomed the invited attendees. The purpose of the conference was to share experimental and theoretical findings to answer the question, "Does Low Temperature Nuclear Change Occur in Solids?" Bockris mentioned that some evidence has been gathered. For example, 120 papers have reported the finding of tritium produced from experiments using palladium. Here are the presentations made at this conference:

**PAPERS PRESENTED ON BASIC EXPERIMENTAL STUDIES**

*Dr. Thomas O. Passell (Electric Power Research Institute, Palo Alto, Calif.), "Overview of EPRI Program in Deuterided Metals."*

Passell took this opportunity to report on funded research conducted at Texas A&M under the direction of Dr. Kevin Wolf. [See *IE*#2, pp. 30-32] Three cold fusion cells containing small amounts of boron and aluminum in a 0.1 Molar LiOD heavy-water reactor were connected in series and operated at a constant low current for 42 days. On the 21st and 22nd day of the experiment two successive fast neutron episodes were observed at about two times background. After the completion of the experiment, the three 6mm dia. by 60 mm palladium cathodes were found to be mildly radioactive. Upon analysis it was found that all three cathodes contained about 100 billion atoms of Ag, Pd, Rh, and (in one cathode) Ru isotopes having ratios unlike ratios that could have been obtained from bombardment by high energy deuteron or proton beams. This later measurement was made to ensure that no one would claim that these isotopes could have come from inadvertent use of high-energy bombardment. Postulated nuclear reactions that could have occurred are: Pd-108(d,$\gamma$)Ag-110 or impurity Ag-109(d,p $\alpha$)Ag-110; Pd-102(p, $\alpha$)Rh-99; Pd-106(d,p $\alpha$)Ru-103 or Ru-102(d,p)Ru-103; Pd-105(d,n)Ag-106 or Pd-105(p,$\gamma$)Ag-106; Pd-104(d, $\alpha$)Rh-102 or Pd-105 (p, $\alpha$)Rh- 102; Pd- 104(p, $\alpha$)Rh- 101; Pd-104(d,n)Ag- 105 or Pd-104(p, $\gamma$)Ag-105.

Note that only two of these 12 possible reactions produce neutrons. This experiment has been repeated but not with successful replication as yet.

If this work is replicated, the tentative conclusions are: Neutron capture is precluded as the primary mechanism because these isotopes lie on the neutron deficient side of isotope stability. Deuterons or protons must have somehow entered palladium nuclei with the subsequent emission of alpha (He-4) particles, protons, gammas, and neutrons. Protons and deuterons can enter palladium nuclei. Similar effects [of proton capture] should be sought in light-water experiments. These type of nuclear reactions can be the source of excess heat. The following tentative conclusion: "Since the activation levels in Pd are 2 or 3 orders of magnitude above detector background, this phenomenon provides an unequivocal nuclear signature observable outside the cells because of the high penetrating power of gamma rays." [As cited by Robert Bass, in his ICCF5 paper, some type of reactions in a metal lattice cannot emit gammas but the energy is absorbed by the entire crystal. —H.F.]

*Dr. T. Ohmori (Hokkaido Univ.), "Iron Formation in Gold and Palladium Cathodes."*

In experiments, excess heat has been obtained from gold, silver, nickel, and tin cathodes in light-water reactors with excess power ranging from 0.2 to 1.0 watt. Some iron was produced using a gold electrode with the amount of iron being a function of the excess heat produced. The iron isotopes depart from the usual isotope ratios found in nature, therefore contamination was not a source of the iron. It was found that sodium sulphate was a better electrolyte than sodium carbonate (with same 0.5M and same current). Examination of the gold cathode showed that the iron content depleted rapidly with depth below the gold electrode's surface. The ratio of Fe-57 to Fe-54 was much greater than the natural ratio.

*Dr. Y. Kucherov (ENECO), "Material Analysis after Glow Discharge Experiment."*

The glow-discharge plasma experiments used about 1 milliampere of current at voltages of 100 volts and higher. From theoretical considerations a variety of elements have been projected to be produced and such elements have been found in the palladium cathode after experimental use in the glow- discharge reactor. Admittedly, there are some sources of contamination for some of the observed elements, but not for all of the elements found. The conclusion is that there appear to be elements that can only be created by nuclear reactions and that it appears that both fusion and fission reactions must be occurring in the glow-discharge gas-plasma reactor using deuterium gas and palladium cathodes.

*Dr. John Dash (Portland St. Univ.), "Microanalysis of Pd Cathodes after Electrolysis in Aqueous Acids."*

Dash and his graduate students have used two identical electrochemical cells in series. All of the features of each cell were the same except that one cell contained heavy water and the control cell contained light water. The electrodes used were platinum and palladium with a sulfuric acid electrolyte. After operating the cells for a minimum of five hours, an SEM analysis is performed on the cathodes to determine the experimental results. Spots of activity are observed on the heavy water cathodes. When examined it is found that silver and cadmium are present in the active areas. One experiment was run for four hundred hours.

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There were large concentrations of gold in spikes protruding from the palladium electrodes in both the light and heavy water cells. Careful observation of the same electrode surface without further electrolysis showed that elemental changes still continued. In other experiments using titanium cathodes for 50 hours of electrolysis the excess heat raised the temperature of the active cell four degrees higher than the control cell. Post experimental examination found Cr, Fe, and Ca in one spot on the titanium cathode.

*Dr. Robert Bush (Cal Poly, Pomona), "Electrolytically Stimulated Cold Nuclear Synthesis of Strontium from Rubidium."*

Bush reported on the work that he and Professor Robert Eagleton have accomplished in an ingenious light-water cold fusion experiment. The isotopic abundance of rubidium is well known. If rubidium can be changed to strontium by proton capture in an electrochemical cell, the isotopic ratio of the strontium would be different than the natural isotopic ratios. This elegant experiment was conducted, mass spectrometer measurements were made, followed by chemical analysis in which the isotopes of strontium were separated and measured. The end result conclusively proves that the strontium (which is not present in either the nickel electrode, nor in the electrolyte) is present on the surface of the nickel cathode after the experiment was conducted. In addition, the isotopic ratio clearly confirms that the strontium could not be explained by contamination! The mechanism ap-pears to be the entry of protons into the rubidium nuclei to create strontium elements.

*Dr. Reiko Notoya (Catalysis Research Center, Hokkaido Univ., Japan), "Low Temperature Nuclear Change of Alkali Metallic Ions Caused by Electrolysis."*

Notoya reported on her light-water experiments in which before and after chemical determinations were conducted. Reportedly, some potassium from the electrolyte is changed into calcium; Cs-133 produced an element of mass 134; and sodium-23 became sodium-24. Notoya apparently uses an open cell type of experimental light-water reactor.

*Dr. Georgiy S. Rabzi (Ukrainian Int'l. Acad. of Original Ideas), "Mechanism of Low Temperature Transmutation."*

Rabzi read a paper in Russian and it was translated for the attendees. An English translation was copied and handed to all attendees. Here are some of the salient concepts from the paper by Rabzi: During his presentation Rabzi passed several samples to be viewed by the attendees. These included a steel nut which acquired the color of copper and was reduced in size; magnetic stainless steel turned non-magnetic; and asbestos which became like a ceramic. [He said] these are all the result of low-temperature transmutation. Certain forces are used to trigger nuclear reactions. These forces are combined geo-electric and artificial fields and control of temperature. Experiments were begun in 1958 to observe transmutations in solids and liquids directly. The results are somewhat akin to cold fusion reactions but are not the same.

In the years of experimenting it was found that various electrodes were best for various materials. Electrodes of copper, aluminum, titanium, and stainless steel have been used. Materials that have been used in experiments include zinc, carbon, graphite, lead, table salt, silica sand, fuel oil, etc. As an example of a typical experiment a sample of 99.5% lead was treated on March 15, 1994 at 650 °C and yielded Pm, Cd, Ge, Ag (0.25%), and Au (0.21%). In an August 1, 1994 experiment, silver at the level of 3% was obtained from the 99.5% pure lead. Rabzi states that no radioactivity has been observed in any of their experiments. In fact, the claim is made that radioactive atomic wastes can be stabilized.

The many years of experiments were performed to obtain an insight into the origin of the processes involved, be the processes transmutation or cold fusion. This search has led to a better understanding of the earth processes in the changing of various elements including the energy-producing fission of uranium.

After presenting the basic concepts of his theory in the paper, Rabzi closed with the following: "I hope to have introduced by my theoretical study a fresh understanding of cold fusion as an essentially natural cold fission, a mere stage in the universal transmutation." The full paper will be published in the proceedings.

## PAPERS ON THEORETICAL MODELS

*Yeong E. Kim and Alexander L. Zubarev (Dept. Phys., Purdue Univ., West Lafayette, IN), "Uncertainties of Conventional Theories and New Improved Formulations of Low-Energy Nuclear Fusion Reactions," paper presented by Dr. Kim.*

AUTHOR'S ABSTRACT

We examine uncertainties of conventional theoretical estimates for low energy nuclear fusion cross-section $\sigma(E)$ and fusion rate $(\sigma v)$. Using new formulations based on the optical theorem and the radial distribution function, we derive new improved formulae for $\sigma(E)$and $(\sigma v)$. Our results of the optical theorem formulation for $\sigma(E)$ indicate that a near cancellation of the Gamow factor can occur if the imaginary part of the effective nuclear interaction in the elastic scattering channel has a very weak component with a long finite interaction range. Uncertainties of conventional estimates of the electron screening effect for $\sigma(E)$ are also examined and a new alternative formulation is proposed. Finally, based on a solution of three-body Schrödinger equation and the optical theorem formulation, we derive a new formula for three-body fusion cross-section and rate and compare its predictions with conventional estimates and also with the recent experimental data for three-deuteron fusion reaction.

Yeong Kim's work is best summarized in his own words as follows:

"We have examined uncertainties due to many approximations made in the conventional theoretical formulations of low-energy nuclear fusion reactions and presented new improved formulations which avoid some of these approximations. Some of the new formulations lead to unexpected results. One striking result is a possibility that a near cancellation of the Gamow factor (or Coulomb barrier transparency) cannot be ruled out completely at present. Another surprising result is that the large value of the 3d [three deuteron] fusion rate recently observed in the laboratory beam experiment [of Kasagi, et al.] may be justified theoretically in terms of a solution of the quantum mechanical three-body problem. Since there is still a great deal of uncertainly and absence of replicable reproducibility at a desirable level of about 100% for anomalous effects, it is at present premature to make definitive tests and comparison of the predictions of our

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new improved formulations with experimental data."

During his presentation Kim stated that by using the new optical theorem the work is almost model-free and rigorous. During his presentation at the Monaco ICCF-5 conference, Kim showed that the probability of proton capture by higher mass elements was about fifty orders of magnitude larger than proton capture with lighter mass elements which is contrary to what has previously been the scientific belief.

*Dr. Peter Hagelstein (MIT), "Neutron Capture in Low Temperature Nuclear Reactions."*

Hagelstein is a brilliant and persistent cold fusion theorist. In each cold fusion conference he has presented one or more papers that have explored new ways in which the observed experimental results can be explained. In his presentation he stated that there are eight experimental observations that are not accepted by the scientific community. These include energy transfer within the metal lattice and neutron hopping in crystals. Neutron hopping can transfer energy. Resonant emission and neutron capture by another nucleus can explain some of the observed results.

Hagelstein discussed the concept of a phonon laser and showed how such a phenomenon may be a part of the cold fusion experiments. In addition, he showed that thermally-induced neutron-hopping can occur with elements having ls orbitals (H and He), 2s orbitals such as silicon, and 3s orbitals (Sn, Cd, Te, and Xe). He concludes that in metal hydrides neutron hopping appears to be feasible. [Neutron hopping with a neutron being captured by the nuclei of a metal lattice would, of course, explain some of the experimental results of observed changes in isotopic ratios in palladium during electrolysis. This result is a form of transmutation.]

*Dr. Robert Bush (Cal Poly, Pomona), "Can the Electron Catalyzed Fusion Model (ECFM) Account for Light Water Fusion?"*

Bush cites Hal Puthoff's paper in which Puthoff shows that the stability of the hydrogen atom is a byproduct of the zero-point energy (ZPE) field. Bush suggests that if the ZPE is distorted by something, then the electron could go into a higher energy state and thus give off energy. Bush shows that the maximum tritium production in a heavy-water cold fusion cell occurs at about 0.825 D/Pd ratio. Bush's theory shows that the tritium production function is a 12th-power function and therefore can change abruptly with loading. His theory also explains the rollover of the excess heat in light-water nickel cathode environment by explanations similar to those used to explain the tritium production.

[Bush's latest theory has been criticized as being too empirical. However, the theory certainly deserves a careful consideration in our search for the parameters that are important in the production of excess heat. —H.F.]

*Mr. R. Davis, "General Explanation of Radioactivities in the Experiments of Deuterided Metals."*

Davis presented the theoretical work of Ron Brightsen in this co-authored paper. A part of the theory explains why the numbers 20, 50, 82, and 126 are special. Davis also handed out a paper, "Application of NCM to Experimental Results," where NCM stands for Nucleon Cluster Model. [See accompanying article in this issue of *IE*.]

**PAPERS ON INNOVATIVE APPROACHES**

*Dr. T. Mizuno (Hokkaido Univ.), "Analysis of Elements for Solid State Electrolyte in Deuterium Atmosphere during Applied Electric Field."*

Mizuno used an alternating current field of 5 to 45 volts and a temperature of 400° to 700°C to produce a few watts of excess thermal power. As a result of these experiments several new elements (not present in the initial materials) were observed. The basic mixture used is a combination of Sr, Ce, Nb and oxygen, with other impurities about 38 parts per million. After a process of sintering, refining, sintering, and plating, this type of proton conductor was used in the experiment at low pressures and with 5 to 45 volts potential. Over a period of 5 to 6 hours excess heat was observed. Analysis of the material using SIM showed that several elements were produced including Al, Bi, Sm, Gd, and Dy.

*Toby Grotz, Dr. Timothy A. Binder & Ronald J. Kovac (Univ. of Sci. & Phil.), "Experimental Examination of Russel's Theory of Transmutation."*

About one hundred years ago, Russel claimed to produce transmutation. Grotz, Binder, and Kovac have been attempting to replicate Russel's work. This presentation showed that they have had modest results in such replication. Binder briefly presented Russel's table of elements and its structure. Grotz reported on their experiments in which they produced some fluorine from water vapor using electric and magnetic fields. Ron Kovac showed the results of some of his vacuum plasma studies in which he has produced an element having a mass of 5 (which is not usually presented in the mass spectrographic charts). These experimenters are some of the few who have deliberately tried to establish the serious nature of transmutation of elements and have had experimental successes.

*Dr. Thomas Claytor (Los Alamos Natl. Lab.), "Tritium Production from a Low Voltage Deuterium Discharge on Palladium and Other Metals."*

Claytor has been working for several years - trying to find relatively low energy methods to produce tritium. Tritium has been produced by bombarding heavy water with floods of neutrons from an atomic reactor, a very expensive and environmentally hazardous pro-cess. Tritium is a key ingredient in hydrogen bombs and costs about $32,000 an ounce. Even with the Cold War over, there is still some interest in the production of tritium for military (defense) purposes. Claytor is using a plasma-type device to produce tritium in substantial amounts. At the present time, the production of tritium is still not a resounding commercial success.

**[Editor's Note: A person under contract to the U.S. DOE was present at the meeting and cited the commercial importance of the production of tritium. It is difficult in this time of relative peace to become serious about the production of tritium when the great need for the world is to produce non-polluting energy. —H.F.]**

*Mr. D. Hudson (private researcher), "Orbital Rearrangement of Monoatomic Elements (ORMES)."*

Hudson has been doing intensive experiments with gold, nickel, and copper. He has been able to demonstrate that some elements can be made essentially "invisible" to most chemical measurements and then restored so that the elements are measurable. Hudson showed copies of

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several articles, mainly from peer-reviewed literature and textbooks to indicate that there are some strange results that differ markedly from standard accepted chemistry. For example, Hudson shows that when certain elements are reduced from metals (large arrays of atoms) to states where only a few atoms are clustered, then the normal metallic behavior of these elements is dramatically changed. These experimental results are being investigated by other researchers working in the nanomaterials area. Hudson says to get a metal out of the metallic cluster give the element something that it wants more than it wants itself. Two such separation elements are lithium and sodium.

[Hudson has applied for some patents on his findings. His work is unusual and worth further investigation. Some of the materials that he has formed appear to have a beneficial effect on health. His presentation was logical and impressive. The short presentation was inadequate for Hudson to describe all he had discovered during the past several years of intensive research. —H.F.]

*Dr. Roberto Monti (Burns Development Ltd.), "Variations of the Half-lives of Radioactive Elements and Associated Cold Fusion and Cold Fission Reactions."*

Dr. Monti first came to this editor's attention at the ICCF3 at Nagoya, Japan where he handed out a formula for transmutation of some elements by an explosive or "ignition" method. At this conference Monti described his work in which he believes that cold fusion and cold fission are complementary and reversible. He described experimental results in which after "ignition," the chemical composition of the materials was dramatically different than the initial conditions. Of great interest was his reports on work with radioactive materials in which the radioactivity was greatly reduced after ignition.

[Dr. Bockris mentioned the claims that if some radioactive materials were heated to 1,000° to 1500°C that the radioactivity decreased and asked if anyone was familiar with that work. None in the audience was acquainted with such work.]

**General Discussion at Meeting's End**

There was a discussion of producing proceedings of the conference. Hal Fox, President of Fusion Information Center offered to publish the proceedings if the presenters would get papers to him within thirty days. A copy of the proceedings would be given to each person providing a paper and the proceedings would be offered for sale. Mr. Bauer from Burns Developments Ltd. (Canada) mentioned that his company has offered to pay for experiments with radioactive materials to show that the Monti techniques could reduce the radioactivity. He had not had success in contacts with scientific officials in either Germany or in the U.S.

**_IE_ ASSOCIATE EDITOR HAL FOX'S COMMENTS:**

This was an important first in modern science: A serious conference with twelve papers presented by academic or recognized industrial scientists. Four additional important papers were presented by non-academic private experimenters. Thirty-four concerned scientists, researchers, and journalists were invited and 27 were able to attend this international conference. Four came from Japan, one from Canada, one from Italy, and one from Ukraine.

There is no question but that low-energy nuclear change is a controversial topic.

Every paper presented either experimental (12 papers) or theoretical (4) evidence for low-energy nuclear changes within or near the surface of various metals. Much of the evidence is an outgrowth of the international work on cold nuclear fusion. There is no question but that low-energy nuclear change is a controversial topic. Currently accepted scientific theory denies low-energy changes. One of the most prevalent established scientific beliefs is the inability of a charged particle (such as a proton or a deuteron) to penetrate the Coulomb barrier, especially in metals where the atomic model presents a cloud of orbiting electrons that shield the nuclei. Kim has shown, theoretically, that a hydrogen ion is more likely to penetrate into a high mass element (by fifty orders of magnitude) than to penetrate into a small mass element (like lithium). This theoretical discovery, based on the optical theorem, is strongly substantiated by experimental evidence.

The findings from these 12 experimental and 4 theoretical papers are important. All of the diverse experimental papers substantiate the concept that low-temperature or low-energy nuclear changes can occur. This concept is buttressed by new theoretical considerations, especially related to neutron hopping or "proton capture." In furthering our understanding of matter, nuclear changes, and new energy sources, this scientific approach may be the most fruitful area of scientific investigation to be proposed in this generation.

**Proceedings of the Conference on Low-Energy Transmutation are available for $75.00 per copy from:**
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• Dr. Robert T. Bush, CalPoly, Pamona, discusses his experiments with thin-films of palladium which produced excess power at a peak rate of 3000 watts per cc and produced 15 Megajoules (4 kwh) during nearly six weeks of continuous operation.
• Dr. V.A. Tsarev, P.N. Lebedev Physical Institute, Moscow, reviews dozens of successful Russian experiments.
• Dr. John Bockris, Texas A & M, shows calculations of the effects of high fugacities (internal pressures) on the metal lattice and also describes his latest electrochemical experiments, which produced over 30 Megajoules of excess heat per mole of palladium.

**Tape Two—Two, One-Hour Roundtable Discussions**

• "Similarities and Differences in Cold Fusion Experiments," chaired by Dr. David Worledge of the Electric Power Research Institute. Panelists include Drs. Stanley Pons, Hideo Ikegami, Melvin Miles, and X.Z. Li.
• "Cold Fusion 1992: Where Do We Stand—Where Do We Go." chaired by Dr. Martin Fleischmann. Panelists include Drs. Vladimir Tsarev, Fritz Will, Michael McKubre, Giuliano Preparata, Robert Huggins.

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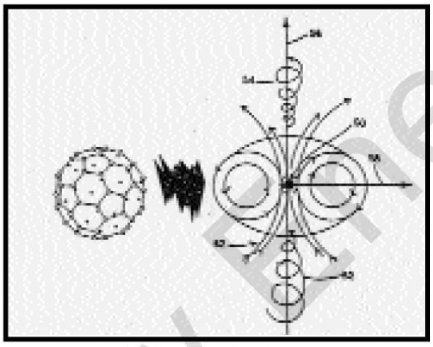

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# Application of the Nucleon Cluster Model to Experimental Results

**Presented at the Texas A&M Meeting on Low Energy Transmutation, June 19, 1995**
**by: Randy Davis for R.A. Brightsen (Author of Concept), Clustron Sciences Corporation, Reston, Virginia**

[*EDITOR'S NOTE: The Nucleon Cluster Model of Ronald Brightsen, correct or not in its description of the structure of nuclei, is certainly one of the most audacious attempts to explain nuclear anomalies. Even if one were to conclude that "antimatter clusters" within the proton and other nuclei cannot be correct, the mathematical structure and formalism of this model deserves to be mined for the clues that it may offer to a new paradigm of nuclear physics.—EFM*]

## ABSTRACT

**According to the Nucleon Cluster Model and the Periodic Table of Beta-Stable Nuclides, the proton must contain antimatter clusters as well as positive matter clusters. Within palladium cathodes, antimatter clusters are strongly attracted to matter clusters of the same type, avoiding the so-called "coulomb barrier." The resulting reactions produce the many different radioactive species recently reported for cold fusion experiments. The $^{104}$Pd (p,$\alpha$) $^{101}$Rh reaction is discussed as an example.**

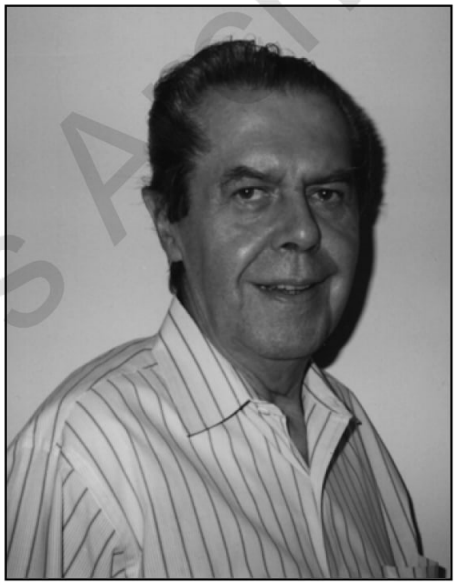
The Nucleon Cluster Model (NCM) provides a pathway for experimentalists to understand the results that they obtain in the laboratory in the study of low temperature transmutations/low energy reactions. In the future, it may permit experiments to be tailored to obtain particular results desired. This briefing (Slide 1) describes one of the ways that the NCM can be applied, by considering the results reported earlier by Dr. Tom Passell in the lead-off presentation of today's meeting on Low Energy Nuclear Reactions. The species that were observed by Dr. Kevin Wolf are explained by the Nucleon Cluster Model as having been produced by matter-antimatter reactions. The attached paper, "General Explanation of Radioactivities Reported by Dr. Wolf as Described by Dr. Passell (EPRI) at Monaco," provides supporting detail. The paper was written by Ron Brightsen of Clustron Sciences, who is the developer of the Nucleon Cluster Model.

Before discussing the physics involved, I would like to say that Ron Brightsen can be called upon to assist you in applying the NCM to your experimental results. As this field of investigation becomes a mature technology, we will also be available in the Washington, D.C. area to assist in developing proposals and programs for federal funding. Mr. Brightsen can be contacted through the telephone and address given, or through my e-mail address (rhame@aol.com).

The NCM was developed (Slide 2) over many years of investigation by Mr. Brightsen. While at MIT in the late 1940s, he became interested in the structure of nuclei due to the theory of Maria Mayer, which indicated that nuclei with certain numbers of protons or neutrons are more stable than other nuclei. The "magic numbers" that indicated greater stability included, for example: 20, 50, 82 and 126. To him, this was a clear indication that nuclei must have some type of structure, rather than simply consisting of neutrons and protons without any particular order. He was also concerned that no beta-stable nuclear species exist for atomic numbers (Z) of 43 and 61, or for neutron numbers (N) of 19, 21, 35, 39, 45, 61, 71, 89, 115, 123, and 147. Shouldn't stable isotopes with all numbers of protons and neutrons be possible if protons and neutrons truly are the basic building blocks of nuclei?

By studying the entire range of approximately 340 beta-stable nuclei, Mr. Brightsen discovered that all stable nuclear species are arranged in a systematic manner around particular nuclear species, having certain mass and charge numbers, which he has identified as "centers of symmetry." Furthermore, the nuclei are arranged in symmetric patterns around these centers of symmetry. Importantly, these symmetric patterns indicate that one nucleus may be related to another nucleus by the addition of a neutron-proton (NP) cluster, or an NPN cluster, or a PNP cluster. This is an important observation. It is not possible to draw this conclusion unless all three relationships are considered. Thus, beta-stable nuclei must consist of NP, NPN and PNP clusters.

Through further research, Mr. Brightsen was able to describe all of the 340 beta-stable nuclei in a completely systematic, periodic, and symmetric manner. He developed a detailed table depicting the centers of symmetry and the numbers of each of the types of clusters (NP, NPN, and PNP) for each nuclide. The results of this effort are described in a paper, "The Nucleon Cluster Model and the Periodic Table of Beta-Stable Nuclides," which is available from Clustron Sciences Corporation (Due to its length, $5 is requested to cover copying and postage). The Periodic Table of Beta Stable Nuclides, given in Figure 21 of that paper, is analogous to the Periodic Table of Elements discovered by the Russian scientist Mendeleev in 1869. It is believed that this NCM description is correct due to the symmetry of nature that it is able to demonstrate. The model is also able to show that "missing" proton and neutron numbers, mentioned above, are only an artifact of the way that isotopes have been described in the past (reference the DOE/GE/Knolls Atomic Power Laboratory Chart of the Nuclides).



Ronald A. Brightsen

Based on this understanding, Clustron Sciences Corporation has filed patent applications on several processes that may be developed through application of the Nucleon Cluster Model. These include: methods for converting radioactive nuclear wastes to short-lived or stable isotopes; methods for converting Pu-239 to U-235 and/or non-fissionable heavy nuclides; and, methods for improving the

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performance of silicon-based semiconductors. Further work with the model is being pursued to explain the origin of the elements and missing mass in the universe.

The importance of the Nucleon Cluster Model to the field of nuclear physics, in general, and low temperature transmutations/low energy reactions, in particular, results from the discovery that low mass nuclei must contain antimatter clusters to various levels, in addition to ordinary matter clusters. In addition, it is evident that any stable or radioactive isotope may be described by several different combinations of matter and antimatter NN, PP, NP, NPN and PNP clusters. Matter--antimatter clusters not only attract each other due to their opposite charges, but when they are identical they annihilate when they come into contact.

Dr. Wolf's measurements of $^{103}$Ru, $^{99}$Rh, $^{101}$Rh, $^{102}$Rh, $^{105}$Ag, $^{106m}$Ag and $^{110m}$Ag in palladium samples are the result of matter-antimatter reactions between palladium isotopes and normal hydrogen (see below). $^{101}$Rh, for example, is produced by hydrogen reacting with $^{104}$Pd through a type of (p,$\alpha$) reaction. In this case, $^{104}$Pd can be considered to consist of 16 NP clusters, 18 NPN clusters, and 6 PNP clusters, along with other possible cluster combinations. Protium (the hydrogen nucleus) can be considered to consist of 5 NP clusters, 2 antimatter NPN clusters, and 1 antimatter PNP cluster. When these interact, $^{101}$Rh is produced with 19 NP clusters, 16 NPN clusters, and 5 PNP clusters, along with $^{4}$He with 2 NP clusters. Again, other cluster combinations are evident for both $^{101}$Rh and $^{4}$He. Similarly, the NCM representation can be developed for the other isotopes measured in Dr. Wolf's experiment.

## General Explanation of Radioactivities Reported by Dr. Wolf as Described by Dr. Passell (EPRI) at Monoco

by R.A. Brightsen, President, Clustron Sciences Corporation

Based on the Periodic Table of Beta-Stable Nuclides, the cluster structures of the six beta-stable isotopes of palladium can be represented as follows:

| | NP | NPN | PNP |
|---|---|---|---|
| 46-Pd-102 --------> | 18 | 16 | 6 |
| 46-Pd-104--------> | 16 | 18 | 6 |
| 46-Pd-105--------> | 15 | 19 | 6 |
| 46-Pd-106--------> | 14 | 20 | 6 |
| 46-Pd-108--------> | 12 | 22 | 6 |
| 46-Pd-110 --------> | 10 | 24 | 6 |

Note constant PNP clusters

According to Dr. Passell, gamma ray spectrographs revealed no fewer than eight (8) radionuclides produced in the palladium cathode by low-energy protons or deuterons:

47-Ag-ll0m, 45-Rh-99, 44-Ru-103, 47-Ag-106m, 45-Rh-102, 46-Pd-100, 45-Rh-101, 47-Ag-105.

Classical nuclear physics insists that, because of the "Coulomb barrier," these reactions cannot take place. But experimental evidence proves conclusively that these radionuclides are produced. How can that possibly be? The Nucleon Cluster Model (NCM) provides the answer. Consider one reaction as an example:

Eq. 1: 46-Pd-104 (proton, alpha) 45-Rh-101

In the NCM, the proton (l-H-l) structure requires the existence of antimatter clusters. Thus, the following five (5) structures can represent the proton:

| NP | NPN | PNP |
|---|---|---|
| 11 | -4 | -3 |
| 8 | -3 | -2 |
| 5 | -2 | -1 |
| 2 | -1 | 0 |
| -1 | 0 | 1 |

Let us examine Equation 1 in terms of clusters.

| | NP | NPN | PNP |
|---|---|---|---|
| 46-Pd-104 | 16 | 18 | 6 |
| + | | | |
| l-H-l | 5 | -2 | -1 |
| | 21 | 16 | 5 |

| | NP | NPN | PNP |
|---|---|---|---|
| 45-Rh-101 | 19 | 16 | 5 |
| + | | | |
| l-He-4 | 2 | 0 | 0 |
| | 21 | 16 | 5 |

It is evident from these structures that the following reactions take place:

1) The 17th and 18th NPN in 46-Pd-104 are annihilated by the two NPN antimatter clusters in the proton, and
2) The 6th PNP cluster in 46-Pd-104 is annihilated by the one PNP antimatter cluster in the proton.

Since postitives and negatives attract, there is no Coulomb barrier for the reaction.

A comprehensive picture of this reaction appears at the top of the next page

Similar analyses have been developed for the other observed radioisotopes.

**Slide-1:**
**APPLICATION OF NCM TO EXPERIMENTAL RESUL**

BACKGROUND

- Clustron Sciences Corporation is familiar with many of the models proposed to explain experimental results that are inexplicable via classical physics
- Purpose —to present the NCM explanation of low energy nuclear reactions.
- Consulting assistance is available for application of the NCM to experiments.

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NCM Applied to Explain one of the Transmutations Obtained by K. Wolf

| 46-Pd-104 | + | 1-H-1 | -----> | 45-Rh-101 | + | 2-He-4 |
|---|---|---|---|---|---|---|

| NP | NPN | PNP | NP | NPN | PNP | NP | NPN | PNP | NP | NPN | PNP |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 10 | 20 | 8 | 11 | -4 | -3 | 13 | 18 | 7 | 8 | -2 | -2 |
| 13 | 19 | 7 | 8 | -3 | -2 | 16 | 17 | 6 | 5 | -1 | -1 |
| 16 | 18 | 6 | 5 | -2 | -1 | 19 | 16 | 5 | 2 | 0 | 0 |
| 19 | 17 | 5 | 2 | -1 | 0 | 22 | 15 | 4 | -1 | 1 | 1 |
| 22 | 16 | 4 | -1 | 0 | 1 | 25 | 14 | 3 | -4 | 2 | 2 |

Clustron Sciences Logo

"A perfectly symmetric, systematic and periodic system has been developed that contains all known beta-stable nuclides, and no anomalies. Thiis model is similar to the conventional neutron-proton model in that the nuclei are still fundamentally described as composed of neutrons and protons, but significantly different in that the "building blocks" of nuclei are certain clusters of neutrons and protons (NP, NPN, and PNP). It seems possible that the Nucleon Cluster Model (NCM) will provide a new model for the origin of the elements (and their nuclides) and in addition it has enormous implications (including probable explanations of "cold fusion" and low energy nuclear transmutations) for physics, chemistry, and related sciences. It represents a major and heretofore unknown improvement in our basic understanding of matter."

**Conclusion of the paper by R.A. Brightsen, "The Nucleon Cluster Model and the Periodic Table of Beta-Stable Nuclides"**

**THE NUCLEON CLUSTER MODEL (NCM)**

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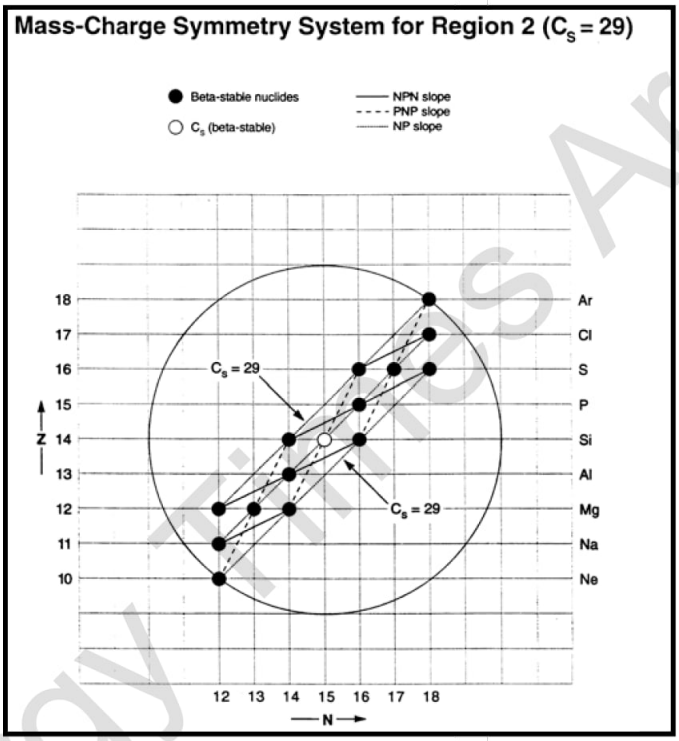
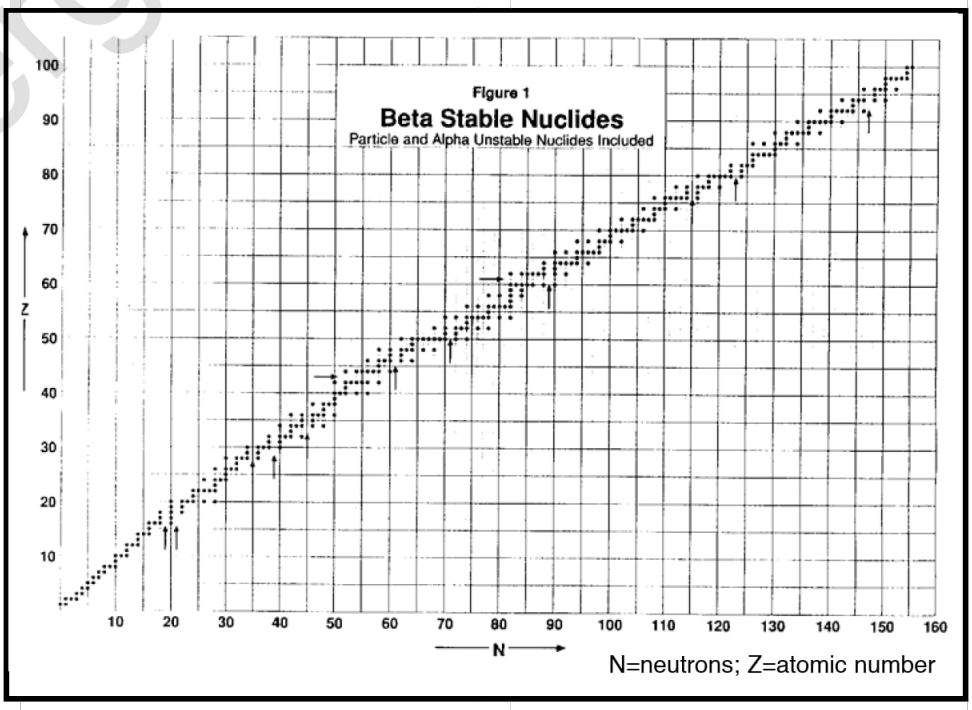
**Slide-2:**

**NUCLEON CLUSTER MODEL**

- Is a completely systematic, periodic and symmetric description of all stable and radioactive isotopes
- The NCM"periodic table of beta stable nuclides" for the first time in the history of nuclear physics incorporates all known beta-stable nuclides (Ref. paper, "The Nucleon Cluster Model and the Periodic Table of Beta-Stable Nuclides," available from Clustron Sciences Corporation.)
- NCM demonstrates that all beta-stable nuclei can be described in terms of three clusters -NP, NPN, and PNP
- NCM makes it possible to explain otherwise inexplicable low-energy nuclear reactions, short-circuiting the coulomb barrier
- Other applications or implications being pursued: converting radioactive wastes to short-lived/stable isotopes; converting Pu-239 to U-235 or to non-fissionable nuclides; improving silicon-based semiconductors; and explaining origin of the elements and missing mass.•

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# Excess Heat in Cavitation Devices: World-wide Testing and Reports

Assembled by Eugene Mallove

## INTRODUCTION

*As many of you know, it has been a long, hot summer here in the U.S. and in Europe. Here in Bow, New Hampshire (near the capital, Concord), it might have been very hot in our temporary non-airconditioned laboratory facility, but it was unfortunately excessively "cold" inside our Yusmar vortex chamber/cavitation tube!*

This component of a commercially manufactured "Thermogenerator" heating system was provided to us by Dr. Yuri S. Potapov of Vizor Corporation of Kishinev, Moldova. Extensive testing of one Thermogenerator here in Bow and even more exhaustive testing by Scott Little at EarthTech International in Austin, Texas, has so far revealed none of the hoped-for and expected excess power. (One other U.S. lab, which prefers to remain anonymous, worked with Dr. Potapov earlier, and apparently also did not see convincing excess power, but we do not have their results.) Our recent experimentation follows the very positive reports of wide-spread commercial acceptance of these thermogenerator heating systems, which were relayed by U.K. Contributing Edi-tor Chris Tinsley and *IE* Advisor Dr. Peter Glück of Romania. (See article in *IE* #2, May/June).

Of course a variety of conditions of water flow rate, inlet pressure, and pipe geometry were tried, in an effort to coax excess heat out of the devices, but to no avail. This does not mean, strictly speaking, that the units as received and as modified were set-up with the proper conditions to obtain excess power. It could be that some necessary condition of resonance or whatever was not achieved, but at this point, as Scott Little points out in one of the following reports, it is now up to Dr. Potapov to be more helpful in assuring proper operation of devices intended to substantiate his claims.

There is at least one report that gives a very positive assessment of Yusmar devices. It too is based on extensive testing, but with a larger variety of Yusmar units. The report is signed by three engineers from a factory in the Republic of Belarus. Our complaint with that report is that it contains ambiguities that are difficult to resolve. A test report from China given to us by Dr. Lev Sapogin, and which we had translated, was inconclusive. Following this introduction, we document as many of these tests as we can fit in. Perhaps more reports will appear in future issues.

As this issue of *IE* went to press, Chris Tinsley was about to depart for St. Petersburg, Russia, to participate in the testing of a fully-equipped factory-delivered Yusmar system—with Russian motor, centrifugal pump, and factory piping. Many of us who participated directly in the arduous tests this summer are waiting with baited breath on the forthcoming St. Petersburg tests. We will report those tests too in *IE* #4.

Apart from the mystery of how a relatively expensive device (compared to an electric immersion heater) could be selling so well in eastern Europe, there is yet another mystery. A former senior U.S. Air Force contract administrator, Mr. Robert Smith of Oakton International in Virginia, saw a Yusmar unit in operation at the cosmonaut training facility near Moscow. He described the sound level of the unit in operation there as "deafening." Both Scott Little and I certify that our devices made no such "deafening" sound. Furthermore, Robert Smith told this editor that there were sharp-edged "cookie-cutter"-like circular devices stacked inside the exit tubes of that Yusmar. He was told that these were designed to make many cavitation bubbles in the water. There are no such devices in either the unit tested in New Hampshire or in Texas, which were obtained though independent channels.

Despite this setback, frustrating mystery, and frustrating dealings with Moldova (and don't ever try to get a box of six Yusmar cavitation tubes through U.S. customs on a hot June day at Boston Logan airport!), we remain very optimistic about cavitation-induced excess energy.

Why? Because there are on record the numerous tests of the Hydrosonic™ pump of Hydro Dynamics, Inc. of Rome Georgia, which reveal (in our view) incontrovertible excess power at the multi-kilowatt level in the range 10% to 30% excess and beyond. That device is so impressive that a team of NASA engineers at Marshall Spaceflight Center has teamed up with Hydro Dynamics, Inc. to further investigate its performance.

The Hydrosonic Pump™ had a precursor, the steam generating cavitation device of the late Karl Schaeffer in the Chicago area. The Hydrosonic Pump patent specifically refers to the Schaeffer device and was likely an outgrowth of it. Michael Huffman of Seattle, who described his plastic cavitation heating device in IE #1, now has a U.S. patent on it—#5,419,306. He remains convinced that it produced thermal power beyond input electric power. Further tests are on hold because his only unit was, according to him, inadvertently ruined at high temperature while under test at an outside facility. Through the courtesy of Michael Huffman, we report in this issue on some of the tests conducted by others of the Schaeffer cavitator. Members of his family apparently had a financial interest in a corporation that tried to commercialize the Schaeffer device.

Cold fusion excess heat, cavitation excess heat, and sonoluminescence are all mysteries that show how very

6 Yusmar vortex chamber/cavitation tubes after spray-painting at Vizor factory in Kishinev, Moldova
Destination: Bow, New Hampshire.

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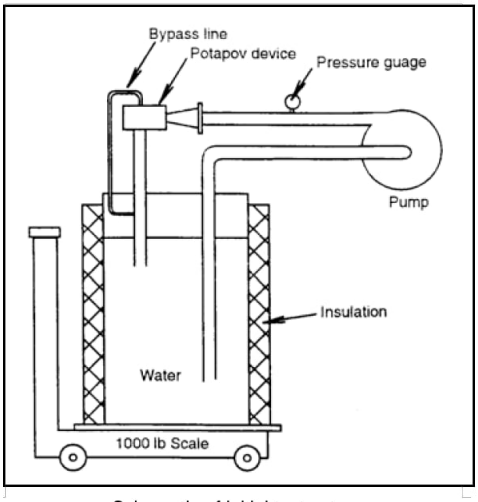


Schematic of initial test set-up
at EarthTech International *Courtesy: Scott Little.*

much more we need to know about these processes within water and other fluids. What, if anything, are the connections among these phenomena? Readers of IE know that we will continue to keep you posted on the latest research aimed at answering these questions. —EFM

*Scott Little and Hal Puthoff prepared five test reports this summer, which are reprinted here:*

## Preliminary Test Results on the Potapov Device

by Scott Little and H.E. Puthoff
EarthTech International

### Introduction

A water-heating device developed in Kishinev, Moldavia by Dr. Yu. S. Potapov has been reported to produce a heat output up to 3 times greater than the energy required to drive it. A Russian physicist, Lev G. Sapogin, has offered a theory to explain this phenomena in his paper entitled, "On One of Energy Generation Mechanism in Unitary Quantum The-ory." We obtained a Potapov device and conducted a series of energy balance measurements on it. No evidence of over-unity performance was observed.

### Apparatus

The device we tested is known as a YUSMAR-2 to the manufacturer. Appar-ently, these devices are enjoying some acceptance in Russia as building heaters and the YUSMAR-2 is the second in a line of four models that they make. For this model, the manufacturer specifies an inlet pressure head of 50 meters of water (71 psi) and a flow rate range of 12.5-23 cubic meters per hour (55-101 gpm).

The device we tested consists of a largely cylindrical chamber with a tangential inlet that spirals gradually into the cylindrical body of the chamber. [See figures on page 20.] There is a main axial outlet that occupies most of one end of the chamber and a secondary, smaller axial outlet on the other end of the chamber. The chamber is 2.5" in diameter and 1" high. The inlet port is rectangular, 1" high and 0.6" wide. The main outlet is 2" in diameter. The secondary outlet is about 3/8" in diameter. Outside the chamber, the inlet pipe is a cone which tapers from 2" pipe down to the rectangular inlet over a distance of 5". The main axial outlet immediately opens into a 24" long section of 2" pipe.

As shown in the figure at left, the device is mounted directly above a steel 55 gallon barrel. The exit pipe from the device was extended with 2" PVC pipe and a "no-hub" coupling to a total length of 38" of which approximately 4" is below the water level. The discharge was placed below the water level to prevent air entrainment in the suction pipe.

The small line marked "bypass" connects the secondary outlet to the main exit pipe. This line appears to be an optional feature of the Potapov device. In the literature accompanying our YUSMAR-2, the bypass line is not shown. However, in a photograph we obtained which shows a device similar in size to the YUSMAR-2, the bypass line is clearly present. We conducted tests with and without this line.

The inlet of the Potapov device is connected directly to the discharge of a 7.5 hp centrifugal pump with a straight 1.5" pipe (discharge port size) 16" in length. 2" PVC piping was used for the suction tube. The total lift in the suction line is about 22". In this configuration, the pump achieves a head pressure of about 60 psi (indicated with a cheap pressure gauge in the dischage line) and a flow rate of about 106 gpm (measured by letting the pump empty the 55 gallon barrel and measuring the fall rate of the water level).

The electrical supply to 7.5 hp pump motor passes through a General Electric 3-phase watthour meter to permit measurement of the electrical energy consumed. This meter has a K of 10.8 which means that the wheel rotates once for every 10.8 watt hours (38880 joules) delivered to the load.

A small immersible pump (not shown in the figure) is used briefly when starting the system to prime the main pump.

The barrel was wrapped with 3.5" thick R-11 insulation batting and is situated on the platform of a 1000 lb scale so the water can be weighed.

A high-accuracy glass thermometer (partial immersion type) with 0.1 degree Centigrade graduations is used to measure the temperature of the water in the barrel.

### Test Procedure

The energy balance measurements were conducted by running the main pump for a certain period and measuring both the electrical energy consumed by the pump motor and the heat energy delivered to the water during that period.

Prior to starting the test, the 55 gallon barrel was filled with ordinary tap water to within about 8" of the top and weighed (the empty weight of the barrel is known).

Before starting the test, the main pump was operated briefly in order to bring the rotating wheel in the watthour meter around so the index mark was centered in the viewing port.

Before starting the pump, the water in the barrel was stirred vigorously


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with a large wooden paddle and a temperature reading was taken by immersing the glass thermometer to the proper depth and waiting until the mercury appeared to be perfectly stationary for at least 30 seconds (i.e. had stopped moving).

To start the main pump, the small priming pump was operated for about 1 minute to fill the piping system and the main pump. Then the main pump was started and the priming pump stopped within a few seconds. While the main pump was operating, revolutions of the watthour meter wheel were counted.

| | Test 1 | Test 2 | Test 3 |
|---|---|---|---|
| starting water temp | 25.60 | 29.60 | 25.95 |
| ending water temp | 29.95 | 33.90 | 27.90 |
| water wt (lb) | 366.5 | 366.5 | 341.5 |
| wheel revolutions | 100 | 100 | 40 |
| bypass line | absent | present | present |
| energy input (Mj) | 3.89 | 3.89 | 1.56 |
| energy output (Mj) | 3.03 | 2.99 | 1.27 |
| * overall efficiency | 0.78 | 0.77 | 0.81 |

The main pump was stopped precisely when a certain number of wheel rotations had been completed and the index mark was again centered in the viewing port on the front of the watthour meter.

Immediately after stopping the main pump, the water in the barrel was stirred with the wooden paddle and another temperature reading was taken in the same manner.

**Results**

Air temperature in our laboratory is typically around 27 degrees C. In test 3, an effort was made to conduct the test over a temperature range that would minimize heat losses to the air (i.e. the water was warmed from slightly below air temperature to slightly above air temperature). Presumably, this is why the efficiency in test 3 is a few points higher than in test 1 and test 2.

Test 2 was conducted about 1 hour after test 1, using the same water. Note that the water temperature fell only 0.35 degrees during that hour.

The pump run in Test 2 was timed at 727 seconds. This time allows the calculation of the average power consumption of the pump motor: 5.35 kW. The motor manufacturer provided a load curve for this motor which indicates that under this loading the motor should be 85% efficient (the efficiency vs load curve is very flat in this loading region).

The errors associated with each of the critical measurements have been estimated as follows:

| | | |
|---|---|---|
| delta-T | 0.05 degree C | (2.5% relative in test 3) |
| water weight | 2 lbs | (about .5% relative) |
| watthours | .5% relative | |

These errors are independent and would thus combine to produce an overall expected error of about 2.6% relative.

**Conclusions**

The Potapov device we tested did not show any evidence of over-unity performance in our tests. The observed efficiency is 4-8% lower than the rated motor efficiency. This difference is significant and is probably due to heat losses to the air and to the body of the pump, which were not measured in these tests.

Our test conditions closely matched the manufacturer's recommended operating conditions for the YUSMAR-2. Our head pressure was about 60 psi instead of the recommended 71 psi but our 106 gpm flow rate was at the high end of the recommended range (55-101 gpm). It therefore does not seem likely that we were "underfeeding" the device.

We can find no explanation for the failure of this Potapov device to perform as reported (300% over-unity). (It is of course possible in principle that we have failed to meet some operating condition that is critical for the over-unity performance.)

*Scott Little, EarthTech International, Inc. 4030 Braker Lane West Austin TX 78759 USA, 512-346-3848 (voice) 512-346-3017 (FAX) little@eden.com (email)*

## Potapov Device Tests - Round 2

5 June 1995, EarthTech International
by Scott Little and H.E. Puthoff

**Introduction**

After our preliminary report was published on the Internet in sci.physics.fusion, a great variety of suggestions, recommendations, and comments have been received. We would like to thank every contributor and assure all of you that we have given each response due consideration in choosing directions for additional testing. Special thanks go to Chris Tinsley for providing first-hand information about the desired operating parameters from his recent visit to Potapov's factory in Moldavia.

We selected temperature, pressure and flow rate as the most important parameters to vary in the 2nd series of tests. We have covered a range of temperatures up to 70 degrees C and a range of flow rates from 54 to 106 gpm. The flow rate was altered by restricting the outlet from the Potapov device which did increase the inlet pressure somewhat.

We have also conducted a control experiment in which the Potapov device was replaced with a simple valve, adjusted to provide a similar pressure drop.

After performing the tests in this report, we discovered that our Potapov device, originally thought to be a Yusmar-2, is actually a Yusmar-1. The Yusmar manual recommends a flow rate of 8.0-12.5 cubic meters per hour (35-55 gpm) and a head pressure of 32-50 meters of water (46-71 psi) for the Yusmar-1.

None of the results obtained thus far show any significant evidence of over-unity performance of the Potapov device.

**Procedure**

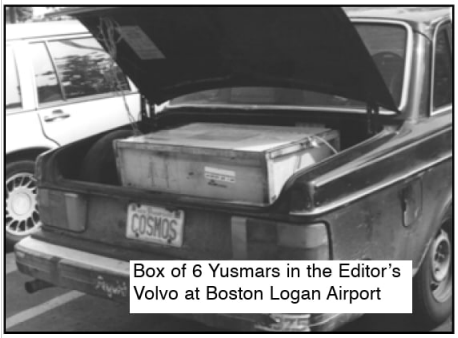
Please refer to the first report for a description of the testing apparatus used in these tests.

As described in the first report, our Potapov device, consists of three major components; a vortex chamber, the main outlet tube (which may function as a swirl chamber), and the inlet transition cone. To restrict the flow rate, we installed an orifice immediately after the vortex chamber, between the vortex chamber and the main outlet tube (see Discussion below).

In all of these tests, the bypass line was absent (see Discussion below).

We conducted tests with two sizes of orifice; 15/16" diameter and 1-1/8" diameter (the main outlet tube has an inside diameter of 2"). The smaller orifice provided a flow of 54 gpm and a pressure of about 67 psi. The larger orifice yielded 65 gpm and about 65 psi.

When the larger orifice was in place,



Box of 6 Yusmars in the Editor's Volvo at Boston Logan Airport

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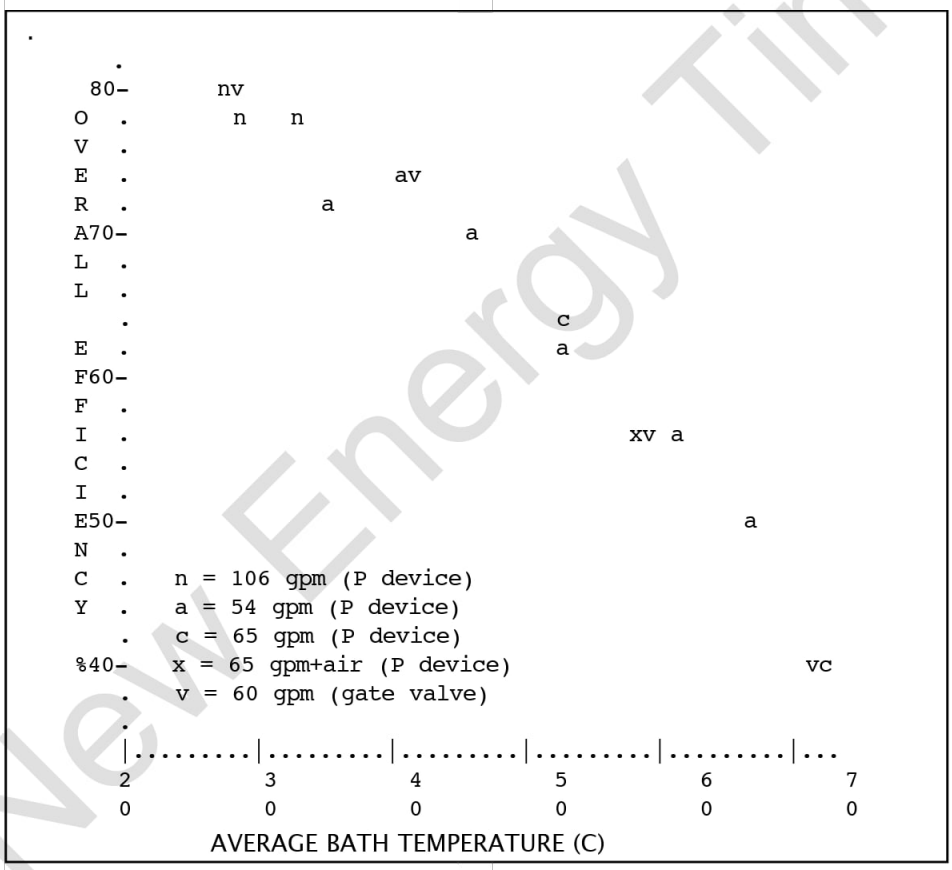
| F | P | $T_i$ | $T_f$ | time | $E_{in}$ | $P_{in}$ | $E_{out}$ | eff |
|---|---|---|---|---|---|---|---|---|
| **No restriction (tests in 1st report) ("n" on plot)** | | | | | | | | |
| 106 | 60 | 25.60 | 29.60 | - | 3.89 | - | 3.03 | 78 |
| " | 60 | 29.60 | 33.90 | 12:07 | 3.89 | 5.35 | 2.99 | 77 |
| " | 60 | 25.95 | 27.90 | - | 1.56 | - | 1.27 | 81 |
| **15/16" dia restriction ("a" on plot)** | | | | | | | | |
| 54 | 67 | 31.62 | 36.62 | 17:00 | 3.89 | 3.67 | 2.83 | 73 |
| " | 66 | 36.62 | 41.40 | 17:43 | 3.89 | 3.66 | 2.87 | 74 |
| " | 66 | 41.40 | 45.85 | 17:52 | 3.80 | 3.63 | 2.67 | 69 |
| " | 66 | 45.85 | 54.05 | 35:52 | 7.78 | 3.62 | 4.92 | 63 |
| " | 65 | 54.05 | 61.15 | 36:14 | 7.78 | 3.58 | 4.26 | 55 |
| " | 65 | 61.15 | 64.35 | 18.25 | 3.89 | 3.52 | 1.91 | 49 |
| **1-1/8" dia restriction ("c" on plot)** | | | | | | | | |
| 65 | 66 | 47.50 | 49.45 | 16:23 | 3.89 | 3.96 | 2.50 | 64 |
| **add 125cc/min air to pump inlet ("x" on plot)** | | | | | | | | |
| " | 55 | 53.6 | 57.00 | 16:28 | 3.89 | 3.94 | 2.18 | 56 |
| **remove air input ("c" on plot)** | | | | | | | | |
| " | 65 | 68.75 | 71.20 | 16:29 | 3.89 | 3.93 | 1.57 | 40 |
| **P device removed, gate valve substituted ("v" on plot)** | | | | | | | | |
| 60 | 65 | 26.35 | 28.75 | 8:39 | 1.94 | 3.74 | 1.56 | 80 |
| " | 65 | 38.30 | 40.50 | 8:41 | 1.94 | 3.72 | 1.43 | 74 |
| " | 65 | 54.80 | 56.45 | 8:43 | 1.94 | 3.71 | 1.07 | 55 |
| " | 66 | 68.00 | 69.20 | 8:50 | 1.94 | 3.66 | .78 | 40 |

we varied one of the tests by bubbling approximately 125cc/min of air into the open end of the suction line. This caused the inlet pressure to drop significantly (due, presumably, to the effect on the pumping efficiency) and it created a fine, milky froth in the collection barrel.

A control experiment was conducted by replacing the Potapov device with a 1-1/2" gate valve adjusted to provide a similar pressure drop.

In all of these tests, we ran the system for a sufficiently long period of time to warm the water bath up to 60-70 degrees C. During the run, observations were taken periodically according to the procedures outlined in the first report.




## Results

In the table below, data from the first report is included for completeness.

The heading abbreviations and units are as follows:

F = flow rate (gallons per minute)
P = pressure (pounds per square inch)
$T_i$ = initial bath temperature (degrees Centigrade)
$T_f$ = final bath temperature (degrees Centigrade)
time = pump running time (minutes:seconds)
$E_{in}$ = electrical energy input (megajoules)
$P_{in}$ = electrical power input (kilowatts)
$E_{out}$ = heat energy collected in bath (megajoules)
eff = overall efficiency (%)

The plot below shows how the overall efficiency varies with the average bath temperature in these tests.

## Errors

The same error considerations discussed in the first report apply to these measurements. Thus the expected maximum error in the observed efficiencies is +/- 2.6%.

A statistical error analysis has not been performed but a rough estimate of the standard error in these energy measurements is 2% relative. It is common practice when measurement errors exhibit a Gaussian distribution to define detection limit as 3 standard deviations. Our excess-energy detection limit, for the overall system, is therefore about 6%. In all of the tests we have performed, a calculation of the flow work done on the Potapov device shows that it is absorbing about 50% of the total energy used to drive the system. This means that the Potapov device would have to produce approximately 12% excess energy for us to observe a 6% overall energy excess. Thus our excess-energy detection limit for the Potapov device is approximately 12%.

When the observed efficiencies are compared to the mean bath temperature (as in the plot above) an additional error due to ambient temperature variations affects the correlation. The magnitude of this error has not been quantified but it should be relatively small since ambient variations are only about 2 degrees C.

## Conclusions

These measurements reflect the overall efficiency of the Potapov device when used to heat a relatively large quantity of water in an open barrel. By overall efficiency, we mean the heat energy added to the water divided by the electrical energy used to drive the system. The steady decline in this effi-

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ciency with increasing water temperature is clearly due to thermal losses which increase directly with water temperature.

The data indicates that our Potapov device, as tested, does not produce anomalous energy in detectable quantity. Within the measurement precision of our tests, the Potapov device heats water just as efficiently as a simple restriction (the gate valve).

**Discussion**

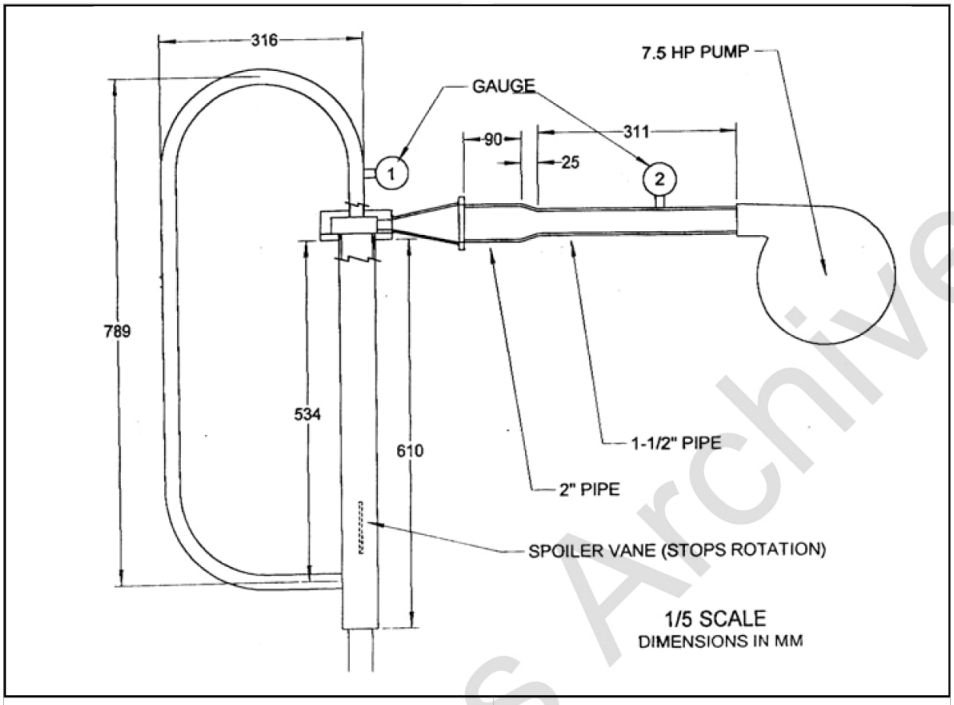
It has become apparent that there are a great many possible combinations of flow, pressure, temperature and piping arrangements that could be tested.

Regarding the piping arrangements, there are three issues that seem most important at this time.

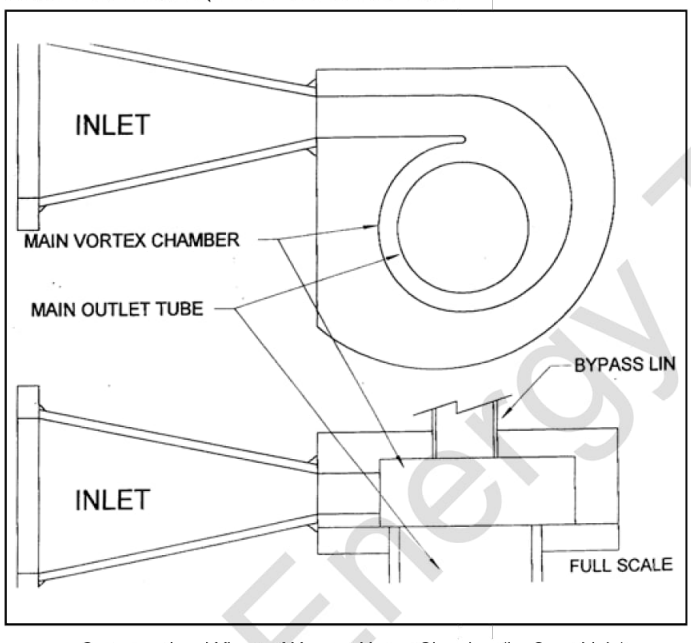
1. It appears likely that the flow restriction should be located not between the vortex chamber and the main outlet tube (as we did in the tests described above) but at the far end of the main outlet tube.

2. The exact configuration of the bypass line is a mystery. We are considering fabricating and installing a bypass line that closely resembles those seen in various photographs of the Potapov device.

3. The arrangement of the circulation loop. It has been suggested that a closed loop (as opposed to our open system) will result in dramatically different pumping requirements which could significantly increase the energy delivered to the Potapov device. We are considering changing to such an arrangement.




Yusmar Test System at EarthTech International Showing Sizes of Components




Cross-sectional Views of Yusmar Vortex Chamber (by Scott Little)

Regarding the pressure and flow requirements, the discovery that we actually have a Yusmar-1 means that most of our tests have been above the recommended flow rates. We will make the necessary adjustments in future tests. It is true that our pressures never quite reach the 50 meter (71 psi) upper figure given in the table of specifications but all of our tests have been within the recommended pressure range (46-71 psi).

This report shows only a set of operating parameters under which the Potapov device does not produce at least 12% excess energy. We continue to seek the correct operating parameters for over-unity performance of our Potapov device. Any such information would be greatly appreciated. We welcome and benefit from criticism of our testing procedures. Please address all inquiries and comments to:

EarthTech International, Austin, TX 78759

**Potapov Device Tests - Round 3**

EarthTech International
Scott Little and H.E. Puthoff
10 July 1995

**Introduction**

For the 3rd series of tests, we modified our Potapov device (a Yusmar-1) by adding a "bypass" line that connects the vortex chamber to the outlet end of the cavitation tube and a diameter reduction at the outlet end of the cavitation tube. Both of these features can be seen in various photographs of the Potapov device (i.e. in *Infinite Energy* #2 and on

John Logajan's web page). We used the photographs and in-formation obtained during Chris Tinsley's visit to Moldova to design these modifications.

As of this report, we have tried one diameter for the bypass line and two different di-ameters for the re-duced diameter outlet. None of the new tests show any detectable over-unity performance from the Pota-pov device.

**Apparatus**

Since this is our third report, a brief review of the apparatus is in order.

The Potapov device is mounted in a vertical orientation (with the vortex chamber on top) above an open 55 gallon barrel. The outlet, which points straight down, is positioned so it is submerged a few inches under the water level in the barrel to prevent air entrainment. The Potapov device, which has a 2" ID inlet port, is fed by a 7.5 hp centrifugal pump. This pump has a 1-1/2" NPT (National Pipe Thread, an American standard) (1-1/2" pipe has a 1.59" ID) discharge

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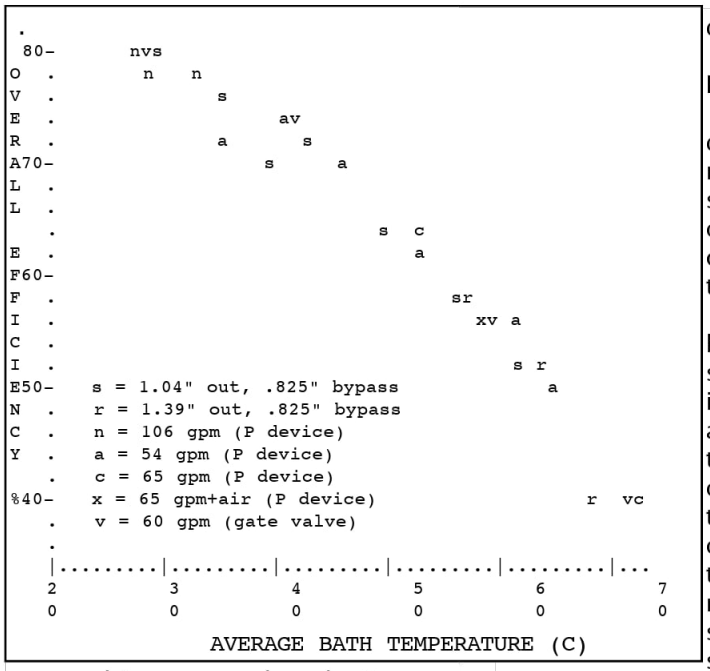


port and is connected to the Potapov device with a straight 17" long pipe section which contains a 1-1/2" to 2" transition fitting. A 2" suction line runs from near the bottom of the 55 gallon barrel to the pump inlet port (which is 2" NPT). The pump must lift water about 22".

The bypass line was constructed from 3/4" EMT thinwall electrical conduit which has an ID of 0.825". A conduit bender was used to fashion the line into a shape similar to that depicted in the photographs. The end of the vortex chamber was drilled and tapped to 3/4" NPT female and fitted with a standard EMT compression fitting with an O-ring in place of the usual ferrule. The other end of the bypass line was connected in a similar manner to the outlet end of the cavitation tube just before the diameter reduction. A 1/8" NPT port was installed in the bypass line very near its connection to the vortex chamber. A pressure/vacuum gauge was installed in this port.

Two diameter reductions were used in the tests in this report. The first group of tests were performed with a 1-1/4" outlet pipe which has an actual ID of 1.36". The second group of tests were performed with a 1" outlet pipe which has an actual ID of 1.04".

Electrical energy consumed by the pump motor is measured with a GE type V-65-S 3-phase watthour meter which we have recently equipped with an electro-optical device to count revolutions of the eddy-current disc. For this particular meter, one revolution of the disc represents 10.8 watthours (38880 joules) of electrical energy delivered to the load.

**Procedure**

The test procedure described in the first report has changed somewhat because of the revolution counter we added to the watthour meter.

Instead of the batch-style measurements described in the first report we are now able to run the pump continuously and simply take readings periodically as the system heats up. Each reading consists of simultaneously measuring the temperature of the water in the barrel and noting the revolution count on the watthour meter. The overall efficiency is then computed using adjacent pairs of these readings. For plotting purposes, the average of the two barrel temperatures is used as the bath temperature.

**Results**

In the data tables below, the various headings are:

# = reading number
count = count of the watthour disc revolutions
T = temperature of the water bath (Centigrade)
time = hour:minute:second clock time of the reading
P(inlet) = gauge pressure in the feed pipe (psi)
P(bypass)= gauge pressure in the bypass line (psi)

With the 1.36" dia outlet pipe, we took the following readings with 358 pounds of water in the barrel:

| # | count | T | time | P(inlet) | P(bypass) |
|---|---|---|---|---|---|
| 1 | 0 | 52.0 | 11:18:08 | 60 | -5 |
| 2 | 90 | 55.0 | 11:29:40 | 60 | -5 |
| 3 | 432 | 65.1 | 12:13:44 | 60 | -5 |
| 4 | 537 | 67.50 | 12:27:20 | 60 | -5 |

Computing the overall efficiencies, average temperatures, and average electrical power consumption for each pair of readings we get:

| pair | efficiency | avg temp | elec. power (kW) |
|---|---|---|---|
| 1-2 | 58.3% | 53.5 | 5.05 |
| 2-3 | 51.6% | 60.0 | 5.03 |
| 3-4 | 40.0% | 66.3 | 5.00 |

The flow rate with this 1.36" dia outlet pipe was measured at 101 gpm. With a 1.04" diameter outlet pipe, we took the following readings with 356 pounds of water in the barrel:

| # | count | T | time | P(inlet) | P(bypass) |
|---|---|---|---|---|---|
| 1 | 0 | 27.3 | 9:37:06 | 64 | +5 |
| 2 | 98 | 31.8 | 9:49:50 | 63 | 5 |
| 3 | 217 | 37.0 | 10:06:00 | 25 | 3 |
| 4 | 287 | 39.8 | 10:19:20 | 24 | 3 |
| 5 | 369 | 43.2 | 10:35:20 | 24 | 3 |
| 6 | 595 | 51.5 | 11:17:00 | 25 | 3 |

Computing the overall efficiencies, average temperatures, and average electrical power consumption for each pair of readings we get:

| pair | efficiency | avg temp | elec. power (kW) |
|---|---|---|---|
| 1-2 | 80% | 29.6 | 4.99 |
| 2-3 | 76% | 34.4 | 4.77 |
| 3-4 | 70% | 38.4 | 3.40 |
| 4-5 | 72% | 41.5 | 3.32 |
| 5-6 | 64% | 47.4 | 3.51 |

Shortly (~20 seconds) before reading 3 was taken, a sudden change in the pump sound was observed (it became noisier). Simultaneously, the feed pressure drop-ped from ~63 psi to ~25 psi and the electrical power consumption of the pump dropped significantly. Apparently, the pump had begun cavitating or had suck-ed in a quantity of air. This new operating condition was fairly stable and readings 4, 5, & 6 were taken with the system in this new state.

After reading 6, we stopped the pump, re-primed it, re-started it, and took three more readings:

| # | count | T | time | P(inlet) | P(bypass) |
|---|---|---|---|---|---|
| 1 | 0 | 52.7 | 11:28:30 | 63 | +5 |
| 2 | 99 | 56.0 | 11:41:40 | 63 | 5 |
| 3 | 241 | 60.2 | 12:00:45 | 63 | 5 |

Computing the overall efficiencies, average temperatures, and average electrical power consumption for each pair of readings we get:

| pair | efficiency | avg temp | elec. power (kW) |
|---|---|---|---|
| 1-2 | 58% | 54.4 | 4.87 |
| 2-3 | 51% | 58.1 | 4.82 |

The flow rate with this 1.04" dia outlet pipe was measured at 99 gpm with the pump operating normally. We were not able to measure the flow rate during the period of abnormal operation.

The graph below shows all our previous results as well as these new results. For the new results, the 1.39" outlet data is plotted with an "r" and the 1.04" outlet data is plotted with an "s."

**Error discussion**

The new measurement procedure is not expected to adversely affect the 2% relative standard deviation estimated in the second report. However, the addition of the bypass line appears to have slightly increased the scattering

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of the data points in the curve above. In the final analysis, it is the degree to which a particular data point fits this curve (or doesn't fit it) that is our indication of anomalous efficiency. Thus a slight increase in our estimate of the minimum detectable amount (MDA) of excess energy is indicated. (In future reports a regression analysis will be performed to determine the actual standard deviation of the data points so a more accurate determination of the MDA can be made).

**Conclusion**

There is no significant tendency for the new data points to lie above the curve defined by the other points. Therefore, our Potapov device still has not produced anomalous energy in detectable quantity.

**Discussion**

Our present flow rates (~100 gpm) are quite a bit larger than those recommended in the instruction book for the Yusmar-1 (33-55 gpm). However, our feed pressure (~60 psi) is not excessively high (the book recommends 46-71 psi for the Yusmar-1). In future tests we will probably arrange an additional restriction in the outlet pipe to lower flow rates while maintaining inlet pressure.

The behavior of the bypass line is interesting. The low gauge pressures ob-served near the upper end of the bypass line indicates that the main vortex is creating a low pressure region in its center. Since the lower end of the bypass line is connected to the cavitation tube just before the diameter reduction (where the pressure should be relatively high), it is a virtual certainty that water is not bypassing the vortex through this line but rather recirculating through this line from the outlet end back to the center of the main vortex. It is possible that this flow of water into the center of the vortex is a vital requirement for the Potapov effect. In future tests we will probably try a smaller bypass line.

**Potapov Device Tests - Round 4**

EarthTech International
Scott Little and H.E. Puthoff
17 July 1995

**Introduction**

For the 4th series of tests, we explored a range of flow rates and we tried a smaller bypass line. No evidence of over-unity performance was observed.

We also installed electrical probes in the main vortex chamber in an effort to detect any voltage being generated across the vortex. Small voltages and currents were observed.

**Apparatus**

The new bypass line was fabricated from 1/2" EMT thinwall conduit which has an inside diameter of 0.62". Like the first bypass line, it was fitted with a port near the main vortex chamber for a pressure/vacuum gauge.

For all of the tests in this report we used the 1.04" diameter pipe for the main outlet.

Expressing these pipe sizes as fractions of the pump's discharge port area A (1.59" dia), we have:

1" outlet pipe: 0.43A
3/4" EMT bypass: 0.27A
1/2" EMT bypass: 0.15A

As reported before, our pump achieves an input pressure of around 60 psi and a flow rate of about 100 gpm with no additional restrictions. In order to limit the flow rate for these tests, we added an adjustable valve at the end of 1" outlet pipe.

The voltage probes were designed to touch the water in the vortex at some distance inside the chamber (i.e. not right against the wall, where the flow velocity would be very low) with a minimal disturbance of the flow. They were constructed by drilling 1/4" diameter holes, axially oriented, through the end wall of the main vortex chamber.

One hole is almost touching the outer wall of the chamber and the other is located at about 1/2 the radius of the chamber. Into these holes we inserted short pieces of 1/4" OD, 1/8" ID vinyl tubing and positioned the tubing so it was flush with the inside surface of the vortex chamber. Into the tubing we inserted 1/8" dia brass rods whose ends had been fashioned into thin blades 1/4" long with tapered leading and trailing edges. The rods were rotated and positioned until the blades were parallel to the walls of the vortex chamber and only the blade-like portion of the rod was protruding into the vortex chamber. The tips of these probes thus protrude 1/4" into the water. The brass rods were very tight in the tubing which provided a reliable leak-proof seal (for most of the tests).

For the electrical measurements we used a Fluke-87 DVM for both the open-circuit (except for 10 megohm DVM input impedance) voltage and the short-circuit (except for the DVM shunt resistance) current.

For comparison purposes only, an immersion heater was constructed from a 4kW residential water heater element (purchased as a replacement element from Home Depot [a store] for $9.50). The element was fitted to the end of a 1-1/2" pipe so it could be lowered into the 55 gallon barrel and positioned near the bottom. This heater was wired through the watthour meter so its energy consumption could be measured.

**Procedure**

The test procedure followed in the energy balance measurements is identical to that described in our 3rd report.

For the vortex electrical measurements we observed (somewhat inconsistently) the voltage and current under various operating conditions during the energy balance testing. We also performed a semi-quantitative scan of voltage vs flow rate by observing the voltage while varying the flow rate all the way from about 100 gpm to 0 gpm using the adjustable valve.

**Results**

In the data tables below, the various headings are:

# = reading number
count = count of the watthour disc revolutions
T = temperature of the water bath (Centigrade)
time = hour:minute:second clock time of the reading
P(inlet) = gauge pressure in the feed pipe (psi)
P(bypass) = gauge pressure in the bypass line (psi)
mV = millivolts read across the voltage probes
? = observation not made

With the 3/4" EMT bypass line (the larger one), and the flow rate adjusted to 54 gpm, we took the following readings with 343 pounds of water in the barrel:

| # | count | T | time | P(inlet) | P(bypass) | mV |
|---|---|---|---|---|---|---|
| 1 | 0 | 32.0 | 15:45:00 | 62 | +45 | -40 to +40 |
| 2 | 36 | 33.6 | 15:51:30 | 63 | 45 | ? |
| 3 | 95 | 36.1 | 16:02:00 | 64 | 45 | ? |
| 4 | 201 | 40.4 | 16:20:56 | 64 | 45 | ? |
| 5 | 283 | 43.5 | 16:35:27 | 64 | 45 | -80 to -95 |
| 6 | 378 | 46.8 | 16:52:42 | 64 | 45 | 140 to 144 |
| 7 | 465 | 49.4 | 17:08:04 | 63 | 45 | 139 to 141 |
| 8 | 555 | 52.0 | 17:24:12 | 64 | 45 | 108 to 109 |
| 9 | 646 | 54.3 | 17:40:32 | 64 | 45 | 320 to 360 |
| 10 | 738 | 56.4 | 17:57:20 | 63 | 45 | ~250 |

Just after observation 10, we attached a 100 ohm load resistor across the voltage probes and read 6 mV. We then tried a 1000 ohm load resistor

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and observed 40 mV.

Computing the overall efficiencies, average temperatures, and average electrical power consumption for each pair of the above readings we get:

| pair | efficiency | avg temp | elec. power (kW) |
|---|---|---|---|
| 1-2 | 74% | 32.8 | 3.59 |
| 2-3 | 71% | 34.9 | 3.64 |
| 3-4 | 68% | 38.3 | 3.63 |
| 4-5 | 63% | 42.0 | 3.66 |
| 5-6 | 58% | 45.0 | 3.57 |
| 6-7 | 50% | 48.1 | 3.67 |
| 7-8 | 48% | 50.7 | 3.61 |
| 8-9 | 42% | 53.2 | 3.61 |
| 9-10 | 38% | 55.4 | 3.55 |

These points are plotted with the letter "e" on the graph below.

For the next series of tests, we installed the 1/2" EMT bypass line. The flow adjustment valve was closed down until the flow rate was measured at 40 gpm. Shortly after making this adjustment, one of the voltage electrodes blew out of the vortex chamber and a powerful jet of water shot up and struck the ceiling of the lab, creating a rather gentle shower that persisted for a few seconds until I could recover my senses and turn off the pump! A simple clamp prevented recurrence of this phenomena. With 337.5 pounds of water in the barrel we observed:

| # | count | T | time | P(inlet) | P(bypass) | mV | μA |
|---|---|---|---|---|---|---|---|
| 1 | 0 | 28.7 | 10:41:00 | 67 | +55 | 400 | 10 |
| 2 | 85 | 32.7 | 10:58:00 | 68 | 55 | 250 | 5 |
| 3 | 260 | 40.5 | 11:31:48 | 67 | 55 | -40 | -.6 |
| 4 | 363 | 44.8 | 11:52:00 | 67 | 55 | 32 | .5 |

On a different day, with 370.5 pounds of water already heated up (with the immersion heater) the following readings were obtained (same bypass line, same flow rate):

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 5 | 0 | 64.1 | 12:12:15 | ? | ? | ? | ? |
| 6 | 58 | 65.2 | 12:22:34 | ? | ? | ? | ? |
| 7 | 175 | 67.2 | 12:43:17 | ? | ? | ? | ? |

Pairing these observations, we get:

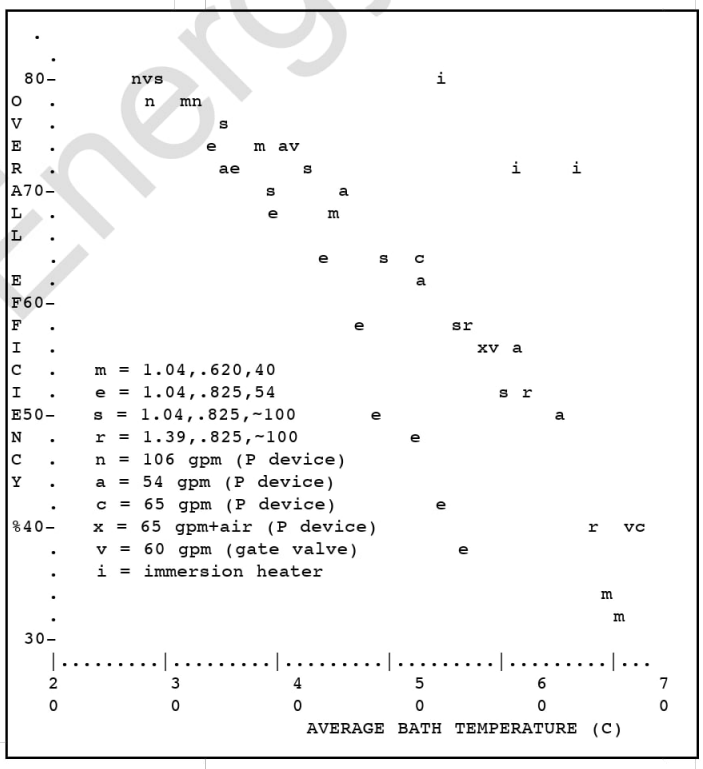
| pair | efficiency | avg temp | elec. power (kW) |
|---|---|---|---|
| 1-2 | 78% | 30.7 | 3.24 |
| 2-3 | 74% | 36.6 | 3.35 |
| 3-4 | 69% | 42.7 | 3.30 |
| 5-6 | 34% | 64.7 | 3.64 |
| 6-7 | 31% | 66.2 | 3.66 |

These points are plotted with the letter "m" on the graph below.

A few of the immersion heater efficiencies are plotted with the letter "i" on the graph below. The numerical data is not given because it is not particularly pertinent.

The three numbers listed in the legend for points m, e, s, & r are outlet diameter (inches), bypass diameter (inches), and flow rate (gpm) respectively.

The semi-quantitative voltage scan mentioned in the procedure section was performed with the 3/4" EMT bypass line installed. We simply watched the voltmeter while slowly varying the flow restriction valve from open to closed. The voltage remained around 70-90 mV throughout the flow range from 100 gpm to 0 gpm.




### Error discussion

It is clear that the combination of lower flow rates and the presence of the bypass line (additional heat loss area) is adversely affecting the overall efficiency. Note in particular the "e" data points in which we had the largest surface area bypass line combined with a relatively low flow rate. This worsening of the correlation between observed efficiency and average bath temperature definitely increases the minimum detectable amount of excess energy. However, if some compensation is made for the lower flow rates when judging these data points, we should still be able to recognize as unusual a point which had, say, 20% excess heat being generated in the Potapov device.

### Conclusion

There is no obvious indication that the new data points represent excess energy production in the Potapov device. If our Potapov device is producing excess energy, it is substan-

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tially less than 20% of the energy required to drive the device.

There are no large voltages or currents generated by the vortex in our Potapov device under the operating conditions tried thus far.

**Discussion**

In the tests performed thus far, we have varied flow rates and aperture ratios rather widely. We have used flow rates at both the low and high end of the recommended range for the Yusmar-1 as well as nearly twice the recommended flow rate. We have tried outlet apertures of .73A and .43A and bypass apertures of .27A and .15A where A is the area of the pump discharge port.

Next we will try an outlet aperture of .50A and a bypass aperture of .20A (precisely the values recommended by Potapov). These ratios will be achieved by inserting machined reducing sleeves into existing pipes (i.e. the 1-1/4" outlet pipe and the 3/4" EMT bypass line).

A remaining question is the preferred orientation of the Potapov device. It is shown in both orientations in the photos in *IE* #2. We are presently assuming that means it must work in either orientation.

The observed voltages were highly erratic, sometimes ranging up to several hundred millivolts but often varying from positive to negative in the 10's of millivolts. Observed short circuit currents were fairly low, never more than 10 microamps. The test involving a 100 ohm resistor produced the highest current, 60 microamps. We did measure the voltage with the Potapov device full of still water and got around 35 mV and .3 microamps, substantially lower than some of the observations when the device was running. This implies that there is some voltage generation mechanism at work in the vortex. Perhaps the erratic nature of our results was caused by the inner electrode not reliably touching the water. Calculations show that the inner radius of our vortex is approximately at 1/2 R, where the electrode is located.

**Potapov Device Tests - Round 5**

EarthTech International
Scott Little and H.E. Puthoff
20 July 1995

**Introduction**

For the 5th series of tests on our Yusmar-1, we adjusted the outlet and bypass apertures to precisely 0.50A and 0.20A respectively where A is the area of the pump discharge port. With these apertures we explored a range of flow rates from well below the recommended minimum to well above the recommended maximum. No evidence of over-unity performance was observed.

**Apparatus**

The original bypass line (made from 3/4" EMT thinwall conduit) was fitted with a machined sleeve with an inside diameter of 0.711" which makes its area precisely 0.20 times the area of the pump discharge port (1-1/2" pipe) which is 1.59" inside diameter. The sleeve is approximately 1.5" long and is located in the very end of the bypass line where it attaches to the main vortex chamber of the Yusmar-1. The rest of the bypass line was left at the original inside diameter which is 0.825".

The outlet end of the Yusmar-1 was fitted with another machined sleeve, also about 1.5" long, whose inside diameter is 1.124" making its area precisely 0.50 times the area of the pump discharge port. This sleeve was located flush with the main diameter reduction so that the entire reduction occurs essentially as a single step.

About 8" beyond this diameter reduction, the adjustable valve described in the last report was attached so that the total flow rate could be adjusted.

**Procedure**

The test procedure followed in the energy balance measurements is identical to that described in our 3rd report.

However, we did make a small alteration to our test procedure to shorten the time required for each series of observations at a particular flowrate. After a given pair of observations were taken (from which an efficiency is computed), the immersion heater was operated along with the Yusmar to rapidly bring the water temperature up to the next desired value. When that value was reached, the immersion heater was turned off and a new pair of observations were made with only the Yusmar device running. In this manner, we were able to measure the Yusmar's heating efficiency over a wide range of temperatures without having to wait for the Yusmar to heat the water all the way by itself.

A casual observation of the voltage developed across the electrodes we installed showed substantially the same behavior as reported previously. We therefore did not record voltages during these tests.

**Results**

In the data tables below, the various headings are:

# = reading number
count = count of the watthour disc revolutions
T = temperature of the water bath (Centigrade)
time = hour:minute:second clock time of the reading
P(inlet) = gauge pressure in the feed pipe (psi)
P(bypass) = gauge pressure in the bypass line (psi)
? = observation not made

With a 26 gpm flowrate and 322 pounds of water in the barrel:

| # | count | T | time | P(inlet) | P(bypass) |
|---|---|---|---|---|---|
| 1 | 0 | 31.85 | 11:09:02 | 69 | +65 |
| 2 | 98 | 36.5 | 11:30:25 | 69 | 65 |
| 3 | 0 | 44.3 | 11:59:50 | 68 | 65 |
| 4 | 104 | 48.3 | 12:23:56 | 68 | 65 |
| 5 | 0 | 60.4 | 12:51:44 | 68 | 65 |
| 6 | 104 | 63.25 | 13:14:50 | 66 | 65 |

Computing the overall efficiencies, and average temperatures for each pair of readings we get:

| pair | efficiency | avg temp (degrees C) |
|---|---|---|
| 1-2 | 73% | 34.2 |
| 3-4 | 61% | 46.3 |
| 5-6 | 43% | 61.8 |

With a 38 gpm flowrate and 324.5 pounds of water in the barrel:

| # | count | T | time | P(inlet) | P(bypass) |
|---|---|---|---|---|---|
| 1 | 0 | 29.4 | 16:20:00 | 65 | +55 |
| 2 | 108 | 34.6 | 16:41:25 | 65 | 55 |
| 3 | 0 | 47.7 | 17:06:21 | 67 | 55 |
| 4 | 107 | 51.5 | 17:28:04 | 66 | 55 |
| 5 | 0 | 59.9 | 17:46:20 | 65 | 55 |
| 6 | 127 | 63.2 | 18:12:00 | 66 | 55 |

Computing the overall efficiencies, and average temperatures for each pair of readings we get:

| pair | efficiency | avg temp (degrees C) |
|---|---|---|
| 1-2 | 76% | 32.0 |
| 3-4 | 56% | 49.6 |
| 5-6 | 41% | 61.5 |

With a 53 gpm flowrate and 347 pounds of water in the barrel:

| # | count | T | time | P(inlet) | P(bypass) |
|---|---|---|---|---|---|
| 1 | 0 | 43.25 | 8:53:20 | 67 | +50 |
| 2 | 127 | 48.2 | 9:16:29 | 67 | 50 |
| 3 | 0 | 60.9 | 9:42:38 | 66 | 50 |
| 4 | 113 | 63.6 | 10:03:22 | 65 | 50 |

Computing the overall efficiencies, and average temperatures for each pair of readings we get:

| pair | efficiency | avg temp (degrees C) |
|---|---|---|
| 1-2 | 66% | 45.7 |
| 3-4 | 40% | 62.3 |

With a 62 gpm flowrate and 343.5 pounds of water in the barrel:

| # | count | T | time | P(inlet) | P(bypass) |
|---|---|---|---|---|---|
| 1 | 0 | 28.45 | 10:28:35 | 64 | +35 |
| 2 | 132 | 34.6 | 10:49:47 | 64 | 38 |
| 3 | 0 | 41.9 | 11:04:32 | 64 | 38 |
| 4 | 143 | 47.6 | 11:27:48 | 64 | 38 |
| 5 | 0 | 61.0 | 11:56:57 | 62 | 39 |
| 6 | 125 | 64.1 | 12:16:56 | 63 | 40 |

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Computing the overall efficiencies, and average temperatures for each pair of readings we get:

| pair | efficiency | avg temp (degrees C) |
|---|---|---|
| 1-2 | 78% | 31.5 |
| 3-4 | 67% | 44.8 |
| 5-6 | 42% | 62.6 |

With a 99 gpm flowrate and 356.5 pounds of water in the barrel:

| # | count | T | time | P(inlet) | P(bypass) |
|---|---|---|---|---|---|
| 1 | 0 | 29.6 | 14:38:55 | 60 | +8 |
| 2 | 156 | 36.4 | 14:59:47 | 58 | 8 |
| 3 | 0 | 43.1 | 15:12:56 | 58 | 7 |
| 4 | 140 | 48.4 | 15:31:55 | 58 | 8 |
| 5 | 0 | 67.4 | 16:08:45 | 57 | 8 |
| 6 | 107 | 69.4 | 16:23:11 | 57 | 8 |

Computing the overall efficiencies, and average temperatures for each pair of readings we get:

| pair | efficiency | avg temp (degrees C) |
|---|---|---|
| 1-2 | 76% | 33.0 |
| 3-4 | 66% | 45.8 |
| 5-6 | 33% | 68.4 |

All of these data points are plotted below along with the data presented in our 2nd report for the gate valve (all the other data points have been removed for clarity).

**Error discussion**

In this series of tests, we made a number of the observations in duplicate in order to check our measurement procedure. The computed efficiencies for these duplicate observations (made as close together in time as possible) typically agreed within 1% relative. Despite this precision, we still observe a significant scatter (visible in the graph above) in the correlation between overall efficiency and average bath temperature due probably to several factors which can vary throughout a given run (e.g. ambient temperature).

We roughly estimate our minimum detectable amount of excess energy from the Potapov device to be in the 15-20% range. Since only about 50% of the total input energy is delivered to the Potapov device in our setup, such an excess would result in an 8-10% increase in the measured overall efficiency.

**Conclusion**

There is no visible tendency for the new data points to lie above the curve defined by the "v" points which were obtained by replacing the Potapov device with a simple restriction. In fact, the new data points tend to lie a bit below that curve probably because of the bypass line which increases the surface area available for heat loss. The bypass line was not present during the gate valve runs.

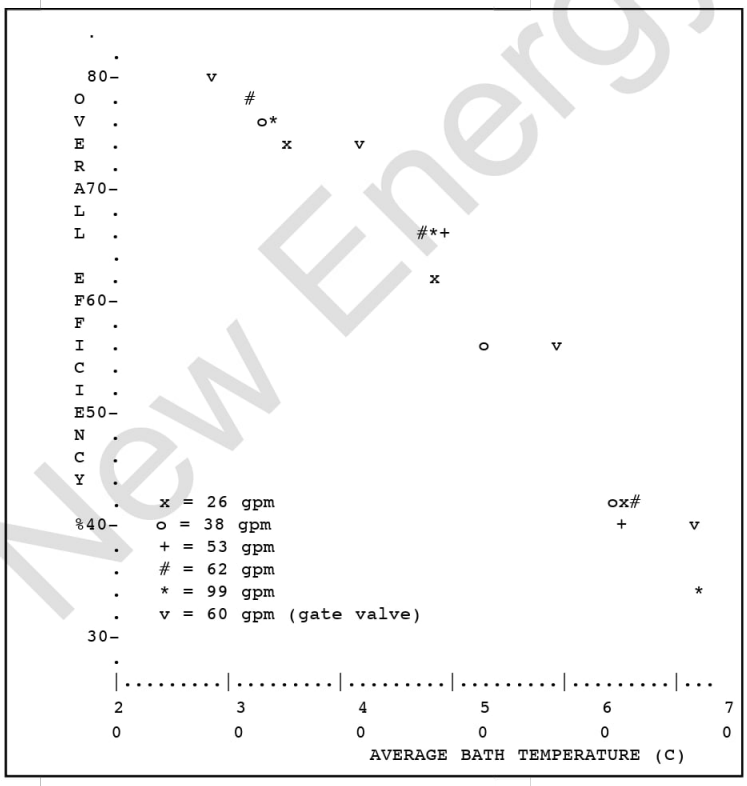
Even though we have followed the flow aperture recommendations of the inventor (Potapov) precisely we still do not see any hint of excess energy even when the device is operated over a range of flow rates that extends well on either side of the recommended range (35-55 gpm).

**Discussion**

At this point, we feel that we have given our Potapov device a reasonable opportunity to show at least a hint of the large over-unity performance that has been claimed. It appears that the device does not perform as claimed but, of course, we could still be missing some critical combination of operating parameters.

Rather than continue searching for such a combination, we will now attempt to communicate our findings to Dr. Potapov and ask for his assistance with further testing.

Scott Little
EarthTech
International, Inc.




## Phase-I Yusmar Testing 6/1/95-7/27/95 Bow, New Hampshire

*by Jeff Driscoll and Eugene Mallove*

Since early June 1995, at a rented industrial facility here in Bow, New Hampshire—affectionately "The Thermogenerator Research Lab" —our group of engineers performed a great deal of work trying to determine if cavita-tion/vortex tubes obtained from Moldova could generate excess power. The facility remained open to the end of August, in the hope that even if the Yusmar technology were found not to evidence excess power, we might test other cavitation type devices here. Some of us are ready to resume testing immmediately if new developments arise. If Chris Tinsley and colleagues, upon travelling to St. Petersburg, Russia in early September, send home the "magic recipe" of conditions that make this work, the now abandoned test rig could rapidly be rebuilt to confirm the approach.

One of us, mechnical engineer Jeff Driscoll, performed most of the work on the project. Next in level of effort has been your dear Editor. Since Gene Mallove is the son and grandson of plumbers and knows something of the trade, that helped! Among other things, he showed Jeff how to "sweat" copper piping.

We also had the able advice and assistance of Don Bullock, also of Bow, NH, a mechanical engineer who has much expertise with pump technology. Don is a world-class expert on reverse osmosis desalination and operates a consulting business in Bow called "The Water Institute." He checked out our industrial facility well water and found it to be of unusually good purity—less than 70 ppm dissolved solids and of pH right on 7.0. Also helping out from time to time was mechanical engineer Jan Roos from Massachusetts—a man who has done a lot of electrical "over-unity" experiments himself, having seen hints of the Genie (yet nothing absolutely solid) over the years.

Initially, we conducted a series of tests using an approach that was recommended by Dr. Potapov when Chris Tinsley and Dr. Peter Glück were in Moldova. This involved putting everything —the Yusmar and a 60-inch tall submersible pump (Goulds Model 48GS50) -- into a large water tank (114 gallons approximate volume). We had to get a tall tank, two plastic bar-

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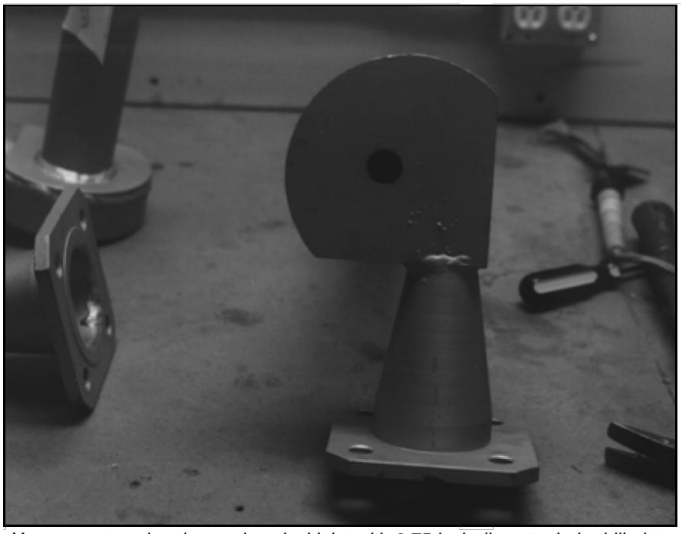


Yusmar vortex chamber and conical inlet with 0.75-inch diameter hole drilled to accomodate threaded fitting for "bypass" tube. *Photo: EFM*

rels that were commercially plastic-welded to-gether to form a cylinder 69.5 inches tall, with an ID of about 22 inches on top (some pinching in the middle and bowing at the mid-section of the two barrels).

We had the tank interior instrumented with eight (8) thermocouples running into a Computer Boards, Inc. interface and then into a 486 PC data acquisition system. We certainly proved that the Yusmar under test—exhausting its fluid directly into the tank, often at 60 gpm, produced no palpable temperature stratification in the tall tank. We also used mercury thermometers as additional reality checks, and I used a hand-held Fluke dual-thermocouple device that confirmed what Jeff was seeing. A ball valve at the end of the Yusmar exit (with a long angle-iron lever to reach it under the hot water) was used to adjust orifice size and the backpressure on Yusmar and ultimately the Goulds pump feeding it.

Our submersible Goulds pump (about 3.8-inch diameter) was jacketed in the tank in a 4-inch PVC pipe, so that its suction at its mid-section would pull water past the Franklin motor that was at the bottom of the pump stack—fast enough to cool the motor. Power into the pump from the three-wire leads plus ground wire coming out the top of the tank was measured by a rented Dranetz 808 digital meter, which told us everything we wanted to know—and more—about the 3-phase, 208V input power. The typical true input power was 4.5 to 5.5 kW, with power factors near 0.80. [All these figures are rough, please! We have printed thermal paper tapes of all runs.]

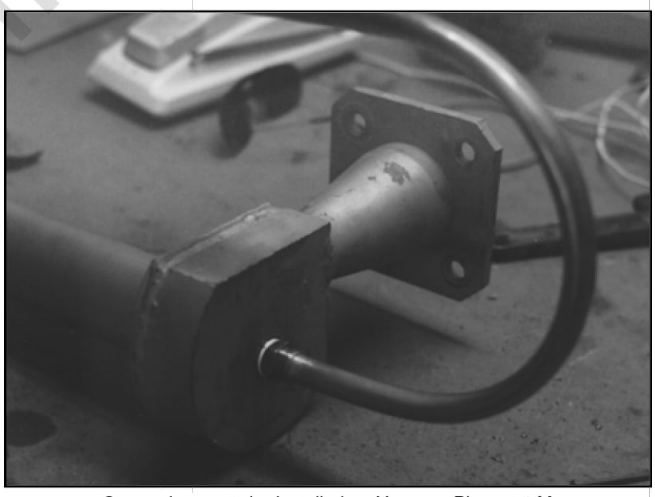
We ran the Yusmar initially with no curved bypass line, just as Dr. Potapov had suggested. Our vortex chamber top hole was too big initially (0.75-inch diameter), but that was later reduced to 0.5 inches and a down-spout was added to divert water into the tank (and away from the ceiling and our faces!). Still later in this initial series, a full by-pass line was added, 0.5-inch copper pipe. Our by-pass line terminated slightly above the "spoiler" plate at the bottom of the Yusmar outflow pipe. Admit-tedly, that was a guess-work choice, so you can see we had many other possiblities that could be checked out, including bypass pipe diameter, for which Scott Little went larger. The bottom line on all these initial tests was that we re-verified the First Law in that tank. Jeff had built a beautiful pentagonal cross-section insulation jacket for the tank: two layers of 2-inch thick Dow foam board. We captured virtually all of the heat from the pump and motor.

We also had a 1-inch ball valve below the Yusmar to vary the back-pressure, which we got up to 120 psi. We were not measuring flow in this initial series. This was "quick and dirty," just to find out whether there was something anomalous that could be seen immediately. We found nothing anomalous, even though we went as high as 90 °C, amazingly, without burning out the pump motor! At the high temperatures, several inches of water evaporated from the top, making the delta-T calorimtery less valid, but that loss could be calculated and all was in rough balance—boring, boring!

**Going to Closed-Loop**

Next, we decided that we had to go closed-loop, in particular, to pressurize the submersible pump inlet and prevent objectionable cavitating behavior in the pump. But we also wanted to see how the Yusmar device worked in air. There was a feeling that maybe the high heat transfer from the Yusmar to the water, vibration damping, or whatever, might quench any unusual mechanisms going on in the Yusmar. So we built the present large enormous rig (see photo next page), which required a scaffolding of 2" X 6" lumber to support. We essentially created a real-world operating heating system. We built the Small Loop (which was not "small" at all) of 2-inch steel pipe to conform with the 2-inch discharge port of the pump. We acquired a used brass, paddle-type flowmeter, which had both mechanical digital readout and a rotating dial that would get us flow-rate within +/- 2 gallons/minute. A 2-inch plumbing flange, by the way, turned out to be a perfect interface for the Yusmar unit, when used with a 0.125-inch thick rubber gasket.

The big problem was cooling our existing submersible pump, we did not want to have to pay for another expensive pump. So we had a local fire-sprinkler system company build us a 4-inch steel pipe water jacket for it, which worked just fine. We had to aquire a 4-inch pipe union to seal it in properly and allow its removal, which we had to do once, unfortunately, after we thought we had fried the motor in hot water (It turned out to be a blown fuse on the wall panel for one phase).



Copper bypass tube installed on Yusmar *Photo: EFM*

Our "Big Loop" consisted of four (4) parallel horizontal coils of 60-feet of 3/4-inch copper pipe (240 feet total), running from vertical inlet and exit manifolds of 1.5-inch ID. All this coilage was immersed in the 114 gallon plastic insulated tank. It turns out that we used this "monstrosity" very little, because we were focusing mainly on the Small Loop testing. We had hoped to run the sequence the way a

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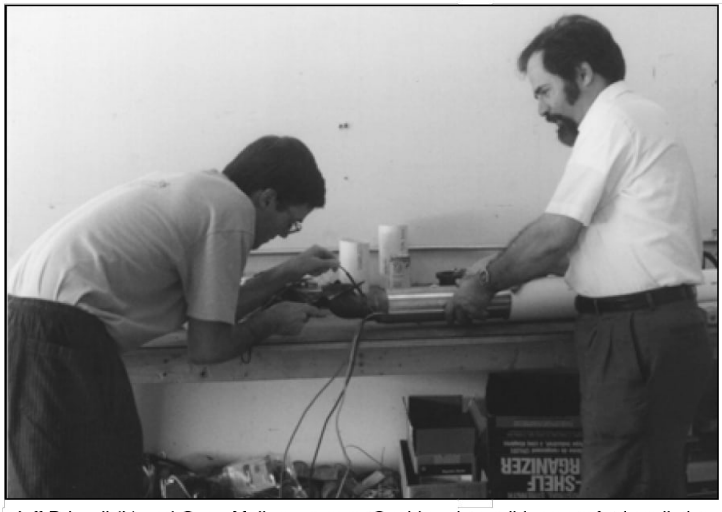

Jeff Driscoll (L) and Gene Mallove prepare Goulds submersible pump for installation in the 114 gallon barrel calorimeter.

real operational Yusmar works, heat up in the Small Loop first followed by heating with the radiator (Big Loop) portion. But as Jeff can attest, it was a marvelous heat exchanger! We could dump most of the heat from an 80+ °C Small Loop into the Big Loop and equilibriate the whole system in less than 10 minutes.

We ran the Small Loop tests hoping to see some anomalous rise. (Initially the Small Loop was not insulated, but later we added lots of foam insulation.) Estimating the internal volume of the Small Loop, we calculated about 50 lbs of water in it. Add to that the estimated 200 lbs of the Small Loop piping and pump/motor weight (the pump/motor alone weighs 70 lbs), and assigning the 200 lbs a specific heat of iron [ $C_p$ (Fe) = 0.13], we could immediately see initially that if there were any excess, it sure wasn't 2X or 3X the 5 kW input power. Remember, since we had a water jacket around the pump and motor, we were capturing their heat, but we were also losing said heat via convection and radiation to the room.

View of the top of the calorimeter tank with pentagonal insulation. Yusmar with bypass tube is to its left. Thick vertical steel casing on the right houses Goulds submersible pump—part of the Small Loop made of 2" pipe. Flowmeter rests on wood frame. Dranetz 808 power meter at far left. Before insulation was put on piping. Copper tubes into calorimeter go to heat exchanger. *Photo: EFM*

We very quickly decided we had to have another testing mode, rather than the absolute form we were using. Thanks to Jeff Driscoll's insistence and his excellent implementation, an in-line reference dum-my pipe was added. This allowed the Yusmar to be tested and the temperature of the Small Loop to be monitored as a function of time (via thermocouples in the flow stream and on its exterior). How-ever, with this in-line system we could also, by valving, switch-in the dummy pipe (which was about as long as the Yusmar), adjust valves to achieve roughly the same pressure, flow, and input power. Direct comparison of the temperature rise profiles would surely allow us to see an anomaly. It turned out that no such anomaly was found in all our tests. A typical profile and analysis follows:

**Sample test:**

The set-up involves the Goulds 48GS50 (5 HP nominal, optimal efficiency 60% at 48 gpm) submersible pump in the small loop with the cooling jacket around this pump. (The Big Loop heat transfer copper tubing in the tank is cut-off by a valve and not used in this form of the test.) There are about 50 lbs of water in this small loop and an estimated 200 lbs of iron/steel.

The small loop is well insulated by foam pipe insulation and reflective foil air-bubble-pack insulation. The exact figures of the thermal mass of the small loop do not matter, however — they do not affect the result, since the comparision test to be described makes the thermal mass irrelevant.

The high pressure water coming from the pump can go either into the Yusmar or into a dummy 2-inch diameter pipe of about the same length as the Yusmar. There is a ball valve immediately after the Yusmar and a gate valve immediately after the dummy pipe. Closing the dummy's gate valve allows the Yusmar to be tested. Closing the ball valve allows the dummy to be tested. The water outflow from either device returns to the pump via the same pipe, so the only difference in the runs is what device is in-line with water flowing through. Since we have tested our thermocouples so well and find that they always agree with one another, for simplicity data was taken from only one thermocouple downstream of either device. We got the following results in a comparision test:

**Yusmar Operated:**
Conditions: about 5 kW in, 118 psi before Yusmar, 75 psi after Yusmar, flow rate at 72-74 gpm

| Time: | Temp (deg C) |
|---|---|
| 11:33 | 21.5 (adjustments being made in valves during first 5 minute interval) |
| 11:38 | 23.4 |
| 11:43 | 30.9 |
| 11:48 | 38.1 |
| 11:53 | 45.0 |
| 11:58 | 51.7 |
| 12:03 | 58.2 |
| 12:08 | 64.3 |
| 12:13 | 70.2 |
| 12:18 | 76.1 |
| 12.23 | 81.7 |

Average temp rise rate = 1.3 deg C/minute

Note the tail-off in temperature rise as the system gets hotter. Implies increas-ed radiation and convection losses.

**Dummy operated:**
Conditions: about 5.1 kW in , 127 psi before dummy, 73 psi after dummy, flow rate = 65 gpm (higher delta-P and lower flow rate, but same power, approximately)

| | |
|---|---|
| 19:28 | 23.7 |
| 19:33 | 31.5 |
| 19:38 | 38.9 |
| 19:43 | 46.0 |
| 19:48 | 53.1 |
| 19:53 | 59.7 |
| 19:58 | 66.0 |
| 20:03 | 71.2 |
| 20:08 | 77.2 |
| 20:13 | 83.0 |
| 20:18 | 88.5 |

Average temp rise rate = 1.3 deg C/minute

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There is remarkable agreement between the two runs. Therefore, there appears to be no significant excess energy inthe Yusmar run.

The actual COP's produced can be calculated from this formula, where Q is the heat increase in the system determined by temperature measurement and the known specific heat of components.

COP (minimum) =
Q(produced)/E(electrical energy into pump)

"Minimum" refers to the fact that the COP is reduced because of inevitable heat losses by this Small Loop calorimeter -- losses by radiation and convection.

COP(min) =
{(mass of $H_2O$) ($C_p$ of $H_2O$) +( mass of steel)($C_p$ steel)}(deltaT)/{(Power ($KW_e$) * time of run *
56.96 BTU/(minute) kW}

$C_p$ (water) = 1 BTU/(deg F* lb)
$C_p$ (iron) = 0.13 BTU/(deg F*lb)

For 50 lbs water and 200 lbs iron, we have:

COP(min) = 2.4 * deltaT/(Power (kW) * minutes)

COP(min) for the Yusmar run = 0.62

COP(min) for the dummy run = 0.61

This is a negligible difference

In any event, it is obvious that if we were getting massive excess heat, like 50% to 100% excess, these COP's should be radically different. A 1.0 from the Yusmar would have provoked sheer joy, all the thermal losses would have been gravy. But the most telling part for now is that in the comparison test the results for temperature rise are essentially the same.

Other tests are quite similar. Below are summaries of conditions tested. Inlet pressure, P1, is pressure a few inces from inlet to Yusmar. Pressure, P4, in table is pressure downstream at pump inlet.

Jeff Driscoll modified the bypass line on the last day of testing to insert pressure gages at the inlet and at the outlet. He found that there was a pressure difference of about 5 psi, with the pressure being lower at the inlet (center of the vortex). So it seems that the flow is indeed from the exit wall of the tube to the inlet.

One strong high-level conclusion comes out of this. We believe that this in-line comparison approach is highly advisable for others to pursue. If this approach is not possible in St. Petersburg, then at the very least a highly accurate measurement of the water content of the system and the mass of the system must be made. If 200 to 300% excess were occurring, we think based on our experience that this would be immediately obvious.

| | P1 (psi) | P4 (psi) | Flow rate (gpm) |
|---|---|---|---|
| Yusmar run #1 | 117 | 78 | 70 |
| Dummy run #2 | 110 | 72 | 72 |
| Dummy run #3 | 125 | 75 | 66 |
| Yusmar run #4 | 120 | 80 | 71 |
| Yusmar run #5 | 110 | 69 | 72 |
| Yusmar run #6 | 142 | 78 | 63 |
| Yusmar run #7 | 165 | 78 | 54 |
| Yusmar run #8 | 223 | 82 | 26 |

Next we present a translation of the Yusmar tests in Belarus:

*Translated from the original Russian text by Valery Milgrom of Concord, NH for Infinite Energy:*

**REPORT**
**In connection with tests for determining effectiveness of heating devices equipped with vortex hydrodynamic heat generators.**

December 1994 through April 1995 there were conducted 25 trial start tests of the devices equipped with the said heat generators at the Bobruisk Machine Manufacturing Plant [Bobruisk, Belarus] experimental site.

Testing rigs included: devices with heat generators based on pumps BK 80-50-200; HK 200-120, heat load connected. Measurements were conducted by regular thermometers for the temperature measurement and wattmeters for the power consumption measurement. Time intervals of the various test stages was registered, too. Temperature of the heat-transfer agent in the case of a Small Heat Circuit [Note: "Small heat Circuit" must mean *without* the radiators connected.—*Editor*] was registered in one place only. Temperature of the heat-transfer agent in the case of a Big Heat Circuit was registered in two places:

Jeff Driscoll manually checks the electric current consumed by one pump motor phase. Small Loop is now insulated and tests are underway. Yusmar and "dummy" are at the top of the insulated pipe riser at the right of the calorimeter tank. *Photo: EFM.*

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inlet and outlet temperatures of a heat consumption apparatus were measured. Effectiveness indexes were determined by calculation using output parameters of the system (temperature, time, the heat-transfer agent mass, the electric drive power consumption). Enclosed is a table listing the results of the tests

**Conclusions**

●**1.** In eight tests, an index "Kilocal. / (KiloWatt * Minute)" in the case of the Small Heat Circuit, without considering a steam generation factor, was in the range 8.0 to 11.6 Kilocal. / (KiloWatt * Minute) that allows [us] to get a Coefficient of Efficiency in the range 1.5 to 2.4 when the device works in an intermittent mode "Heating up Cooling down" -1:2 (in time intervals).

In the rest of the tests, at 4.4 to 7.9 Kilocal. / (KiloWatt * Minute) the Coefficient of Efficiency was in the range 0.93 to 1.45.

In the case of the Big heat Circuit, with the same conditions Coefficients of Efficiency were in the range 2.1 to 10 and 1.2 to 2.0 respectively.

Besides, in two tests good results received on the Small Circuit match good results on the Big one (TG-2 with jet pump and heat generator with adjustable orifice) ["TG" stands for "Heat Generator" -*Ed.*]

In the above stated conditions in the intermittent mode of work, only in one out of twenty-five tests the result [Coefficient of Efficiency] was less than 1.0.

In two cases the Coefficient was 10.0. An average Coefficient for the most characteristic tests (excluding the two most effective ones) turned out to be 1.76.

●**2.** It is believed that the higher indexes of efficiency are typical to the tests when some elements of control of thermodynamic processes in systems were used (jet pumps, hydraulic elevators, adjustable orifice plates).

●**3.** With the lesser capacity of the Small Circuit, the speed of its heating up increases, but the threshold of the pump cavitation decreases.

Any changes in relation of the Small Circuit capacity to the Big one do not affect substantially the speed of the Big Circuit heating up.

●**4.** Increase in the drive power speeds up the Big Circuit heating up, which factor increases the overall effectiveness and efficiency of the system when the device works in an intermittent mode.

●**5.** The higher Coefficient of Performance (COP) is, the higher is the overall efficiency of the device.

●**6.** Decrease in the hydrodynamic resistance in the Small Circuit tubes is appreciable besides factors regulating hydrodynamic characteristics of the current (adjustable orifice plates, etc.)

●**7.** Experiments conducted in the continuous mode did not yield answers for all questions (appearance of additional [ Does this "additional" mean "excess"?-*Editor*] heat in three out of 25 experiments; possible design versions of regulators of the hydrodynamics of the process in the Small Circuit have not been researched in a systematic way).

●**8.** With the increase in pressure in the Small Circuit overall efficiency of the system increases to some degree.

●**9.** Further research with increased ... [Illegible because of poor fax in original Russian - *Editor*] needed.

•**10.** Heating devices with heat generators possess significant advantages over other types of heating devices particularly when economic, maintenance, operational and ecological characteristics are compared. Their Certification for a wide range of use in industrial conditions as well as in private dwellings is recommended.

**Research was conducted by:**
- Chief Engineer of the Bobruisk Machine Manufacturing Plant E.
**Yu. Prokazov (signature)**
- Chief Engineer of the Yurlo Company, Minsk [Capital of the Republic of Belarus]
**G. Zelenko (signature)**
- Leacling Engineer of the Plant,
**Z. I. Gruts (signature)**
- Head of the Plant's Department of Mechanization
**G. D. Belsky (signature)**

**Exhibit to the Report**

1 Kilowatt * Hour = 860 Kilocal. =14 Kilocal. / (KiloWatt * Minute)
[Handwriting on report - *Editor*]

**The Table of Experimental Results:**

**Research of indexes of efficiency of heating devices with vortex and other type heat generators**

**Titles of Columns of the Table**

**1.** Heat Energy of the Small Circuit, Kilocal.
**2.** Heat Energy of the Big Circuit, Kilocal.
**3.** Temperature (delta t1 and delta t2), Celsius degree.
**4.** Pump; COP / (Power, KiloWatt) in the Small and Big Circuits respectively.
**5.** Time, Minutes (T1 for the Small Circuit and T2 for the Big Circuit).
**6.** Heat Energy / (Power * Time) for the Small Circuit, Kilocal. / (KiloWaftt* Min.).
**7.** Heat Energy / (Power * Time) for the Big Circuit, Kilocal. /(KiloWatt * Minute)
**8.** Capacity of the Small Circuit / Capacity of the Big Circuit.

**Titles of Rows of the Table**

[*Describe various Yusmar models and configurations that were tested. — Editor*]

**1; 2; 3; 6:** TG-3, vertical
**4:** TG-3, vertical, with 25 millimeters diameter orifice (or. 25);
**5:** TG-3, vertical, with or. 70 (suction);
**7:** TG-2, vertical, without ...;
**8:** TG-2, vertical, with ...;
9: Valve instead of TG-3;
**10:** Electric Heating Device;
**11:** TG-3, horizontal, with orifice (suction);
**12, 13:** TG-3, horizontal;
**14:** TG-3M ["M" may mean "Modified" - V.M.]
**15:** TG-3M, with or.35;
**16:** TG-3M, with or.25;
**17:** TG-3M, vertical, with or. 35/40;
**18:** TG3.

Note. + means generation of steam was neglected in calculations.

[In labels of rows #7 and 8 "..." stands for an abbreviation I cannot understand. It may be something like "hydroelectric" or "generator of electricity", but I am affraid to specualte - *Translator*]

***The table on the following page is the chart of numbers from the Bobruisk Machine Manufacturing Plant to which the above designated labels pertain:***

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**Bobruisk Machine Manufacturing Plant, Yusmar Test Results**

COLUMN NUMBER

| 1. | 2. | 3. | | 4. | | 5. | | 6. | 7. | 8. | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 2500+ | 11300 | 104 | 30 | 0.5/13.7 | 14.0 | 26 | 118 | 7.3+ | 6.7 | 16/330 | (4.8%) |
| 2000+ | 13000 | 66 | 30 | 0.5/13.2 | 13.8 | 20 | 126 | 7.3+ | 6.5 | 16/330 | (4.8%) |
| 2100+ | 39000 | 79 | 41 | 0.5/13.9 | 13.8 | 23 | 282 | 6.6+ | 10.0 | 16/880 | (1.8%) |
| 11200+ | 13000 | 83 | 31 | 0.5/12.5 | 12.5 | 105 | 210 | 8.5+ | 5.0 | 80/330 | (24%) |
| 2400+ | 11530 | 101 | 31 | 0.5/14.2 | 14.4 | 30 | 98 | 5.6+ | 8.2 | 16/330 | (4.8%) |
| 9000+ | 31800 | 70 | 25 | 0.7/50 | 50 | 25 | 15 | 7.2+ | 42.0 | 80/110? | (7.2%) |
| 10000+ | 63700 | 88 | 54 | 0.7/48 | 48 | 25 | 120 | 8.4+ | 11.0 | 80/110? | (7.2%) |
| 10000+ | 30000 | 83 | 24 | 0.7/37.8 | 42.0 | 32 | 15 | 8.3+ | 48.0 | ??????? | (7.2%) |
| ----- | 36300 | | 44 | | 9.18 | | 600 | | 6.6 | - 1800 | |
| 3000+ | 9130 | 101 | 30 | 0.5/13.2 | 13.8 | 30 | 80 | 7.6+ | 8.2 | 22/270 | (8.2%) |
| 3100+ | 14125 | 110 | 45 | 0.5/14.4 | 14.2 | 35 | 115 | 6.2+ | 8.7 | 22/285 | (7.7%) |
| 2500+ | 47200 | 102 | 39 | 0.5/14.2 | 14.2 | 38 | 237 | 4.7+ | 14.3 | 16/880 | (1.8%) |
| 2400+ | | 84 | | 14 | | | 20 | | 8.6+ | 20/330 | (6.1%) |
| 2500+ | | 92 | | 14 | | | 25 | | 7.2+ | | |
| 2600+ | | 94 | | 14 | | | 25 | | 7.5+ | | |
| 1320+ | | 66 | | 0.5/14 | | 15 | | 6.3+ | | 20/330 | (6.1%) |
| 3200+ | | 127 | | 12 | | | 60 | | 4.4+ | 20/330 | (6.1%) |
| 2500+ | | 88 | | 12 | | | 30 | | 7.?+ | | |
| 2100+ | | 71 | | 13.2 | | | 85 | | /.9+ | | |
| 11300+ | 13000 | 84 | 32 | 13.5 | | 85 | 172 | 9.8+ | 5.8 | 80/330 | (24%) |
| 11000+ | | 81 | | 13.65 | | | 70 | | 11.5+ | | |
| 10000+ | 81000 | 8870 | 0.5 | | 50 | 25 | 25 | 8.5+ | 64.8 | 80/1100 | (7.2%) |

## Other Cavitation Reports: The Schaefer Steam Generator

The Yusmar devices, as we have said repeatedly, are not the only cavitation-type machines that claim to produce excess energy. The patented machine of Carl Schaefer (U.S. Patent #3,791,349) preceeds Potapov and the verified HydroSonic Pump™ of James Griggs (U.S. Patent, #5,188,090) also do. In future issues of *IE* we will examine these machines in much greater depth.

Michael Huffman, who has built his own cavitation-type excess heat producer, recently provided the Vortex-1 forum with his appraisal of the validation of the Schaefer steam generator. We have examined these reports and concur with Michael Huffman's assessment of the Schaefer test reports.

Here is a condensed version of what Michael Huffman wrote:

"I have two reports done by two university professors on two different versions of the Aqua machine.

"As some of you may know, Carl Schaefer [of Chicago] patented his device in 1973. He reportedly spent about 5 million dollars developing and marketing the device. He faced a great deal of resistance from the academic community, and never was able to commercially manufacture the device. He ended up selling the patent for the device in 1988 to the Aqua Corporation in Chicago for $1.00, shortly before he died. Bob Price was an engineer who had worked with Schaefer on the device for 12 years. He had his own machine shop, and was able to design, build, and test his own prototypes.

"He went with the Aqua Corporation and together they spent an additional $500,000 developing and attempting to market the device. Again, they faced a great deal of skepticism, and were unable to successfully market the device. Their main problem was, however, that they were asking 50 million dollars for a technology that would become public domain in a couple of years. Grant Stouffle was the CEO of ill-fated Aqua, Inc., and in my opinion, not a very realistic businessman. Even though they got some serious, multi-million dollar offers from some large capitalization, international corporations, Grant believed the technology was worth more than that, and managed to convince the stockholders that they should hold out for more money.

"When Bob Price's wife died, he left Aqua Inc. to live with his family in another state. Sheldon Hughes joined the Aqua Corporation as an engineer in 1990, and helped develop the device further. He didn't have a machine shop, and the Aqua Corp. ended up spending between 10 and 50 thousand dollars per unit to have them made. Sheldon was working on a deal with the Swenson Corporation, a manufacturer of multi-effect evaporators, to make and test prototypes. That was where I saw the machine that I reported on in the March/April edition of *Infinite Energy*. The Aqua Corp. ended up running out of money shortly after that, and no more prototypes were ever built. The Aqua Corp. then formally folded.

"The first report that I have from the Aqua Corp. was titled, *Preliminary Report on Aqua Incorporated Rotary Impeller Water Heater*. It was written

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by the Chairman of the Department of Physics, North Park College, North Park, Illinois, Linda McDonald, on May 5, 1989. The report is written very much like Scott Little's report on the Yusmar. The report is nine pages long. There is an introduction which defines the variables and formulae, a description of the physical characteristics of the apparatus, a description of the testing procedure and instrumentation, a discussion of the error analysis, a page of data collected from the 10 test runs performed in three temperature ranges, a conclusion section, and an appendix.

"The machine that was tested was one of Bob Price's variations of the Schaefer device. The test procedure used was also very similar to Scott's. It was a basic barrel calorimetry type test in which the incoming water temperature was measured against the [elevated] outgoing water temperature; the volume of water, the power consumption, and the time were measured. In the error analysis section, the accuracy of the measurements was calculated to be +/-11%. No mention at all was made of the efficiency of the motor or any motor-related heat losses. The table of data reported explicitly that the efficiency of the device was between 1.09 and 1.30, depending on the output temperature of the run. The operating temperatures ranged between 95 degrees F to 157 degrees F. The lower temperatures gave the higher efficiencies.

"The other university test report is titled *Aqua Motor Tests* by the Institute For Aviation Research, Wichita State University, Wichita, Kansas, and was written by Glen W. Zumwalt, PhD, Distinguished Professor of Aerospace Engineering, dated March 8, 1990. The tests were performed on several of Sheldon's designs. The report is 8 pages long. Much the same format is used for the report, with the addition of two diagrams of the test bed configuration. The data collected and the formulae used were described in even greater detail than the McDonald report. In this report, a paragraph is devoted to the observation of "blue steam." [Exactly what James Griggs sees with his machine.—*Editor*] The testers speculated that the coloration may have been due to the boiling of the grease in the shaft seal. Samples were collected and analyzed with a gas chromatograph, but only pure water was found. They concluded only that the coloration remained a mystery.

"The issue of efficiency was treated in a very interesting manner, however. In this report, at the end of the data tables for each test run, there is the statement 'If the electric motor efficiency is XX.X%, shaft power supplied equals the heat produced.'" The efficiency numbers used varied with the different rotor designs, but some of the numbers exceeded the manufacturer's nameplate efficiency rating, which led to the recommendation by Professor Zumwalt that a dynamometer be used in future tests to verify the data collected in the barrel calorimetry tests.

"The tests performed on the 'Griggs' Gadget' by the Emprise Corporation report an efficiency of 120% for hot water runs, and 140% for steam runs. This report was written by R. A. DuBose P.E., and President of the Emprise Corporation on Feb. 16, 1994. Again, this report relied on the motor manufacturer's nameplate efficiency rating in its' calculations, and recommended that a dynamometer be installed in the test bed to verify the data collected and reported.

"The test procedure was the barrel calorimetry type of test that Jed Rothwell and Gene Mallove described in their reports. Sheldon also wrote a couple of reports to the Aqua Corp. shareholders on the tests performed at the Swenson Corp. testing facility. One report is four pages long, and the other is three pages. Sheldon measured the amount of steam collected, and found that it was far greater with his device than with an electrical boiler. These reports include graphs that plot the horsepower used against the steam volume output. Depending on the amount of horsepower used, the amount of steam collected was 250% to 500% more than an equivalently powered resistance coil boiler system. The minutes of the Aqua Corp. stockholder meetings show that they voted to have Sheldon patent his design, but Sheldon said that they never gave him the money to do it. As a result, he never patented any of the designs that he came up with while working with the Aqua Corp.

"As you can see from all these reports done by some fairly qualified individuals, mostly in laboratory settings, these devices demonstrate overunity efficiency. You may even know some of the people who have made these reports, and you can verify them yourself. Though Schaefer is no longer living, his son is still around. Sheldon Hughes, Bob Price, and Grant Stouffle are still kicking."

**Huffman provided more material in a submission to the "Vortex" forum by on August 11, 1995:**

"I just got a packet of information from my dad containing more information about the devices developed by Aqua, Inc., and Sonaqua, Inc. The first report was made by Raymond E. Ross, and I believe he was a principal of

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Aquasonics, Inc. of Colorado. The report is not dated, but the note at the bottom from my father suggests that Aquasonics had purchased a license from Sonaqua, Inc. (the original Schaefer corporation) to develop and market the Schaefer device. The note further explains that Aquasonics failed to live up to the contract, went bankrupt, and lost its license. The Colorado-based company was marketing the device as a home heating system, and was calling it the 'Delta Tee.'

"Evidently, Raymond Ross installed one of these devices in his home, and took measurements of the performance. There is no description of the device, the testing protocols, or instrumentation used to make his claims. The report was simply a statement, according to my father's note, and a set of preliminary data that was recorded before sending the device to the Battelle Institute in Columbus, Ohio for testing in their lab.

"It is not a formal report. I would call it more of a 'testimonial' than a scientific report, but it does have some very interesting things to say. The report made by the Battelle Institute was not included in the packet. It was supposedly a formal report of over-unity performance that was the result of testing done at the Batelle Institute. According to Bob Price, Aquasonics supposedly paid $40,000 to Battelle Institute to perform these tests. My parents just recently moved from Indiana to California, and my dad is having difficulty locating the formal report. The following is the complete report from Aquasonics, Inc.:

Aquasonics
The Aquasonic's Delta-Tee

In my opinion, the conventional way of estimating B.T.U output (G.P.M. x delta-T) cannot be used for our unit. I think we have proved this at the Ross residence in Broomfield. By the conventional way of estimating B.T.U. output, we are heating a 1,440 sq. ft. house with a full basement, or a total of 2,880 sq. ft., to 75 degrees F with the outside temperature at 0 degrees F with less that 20,000 B.T.U.H [Btu/hour—Ed.].

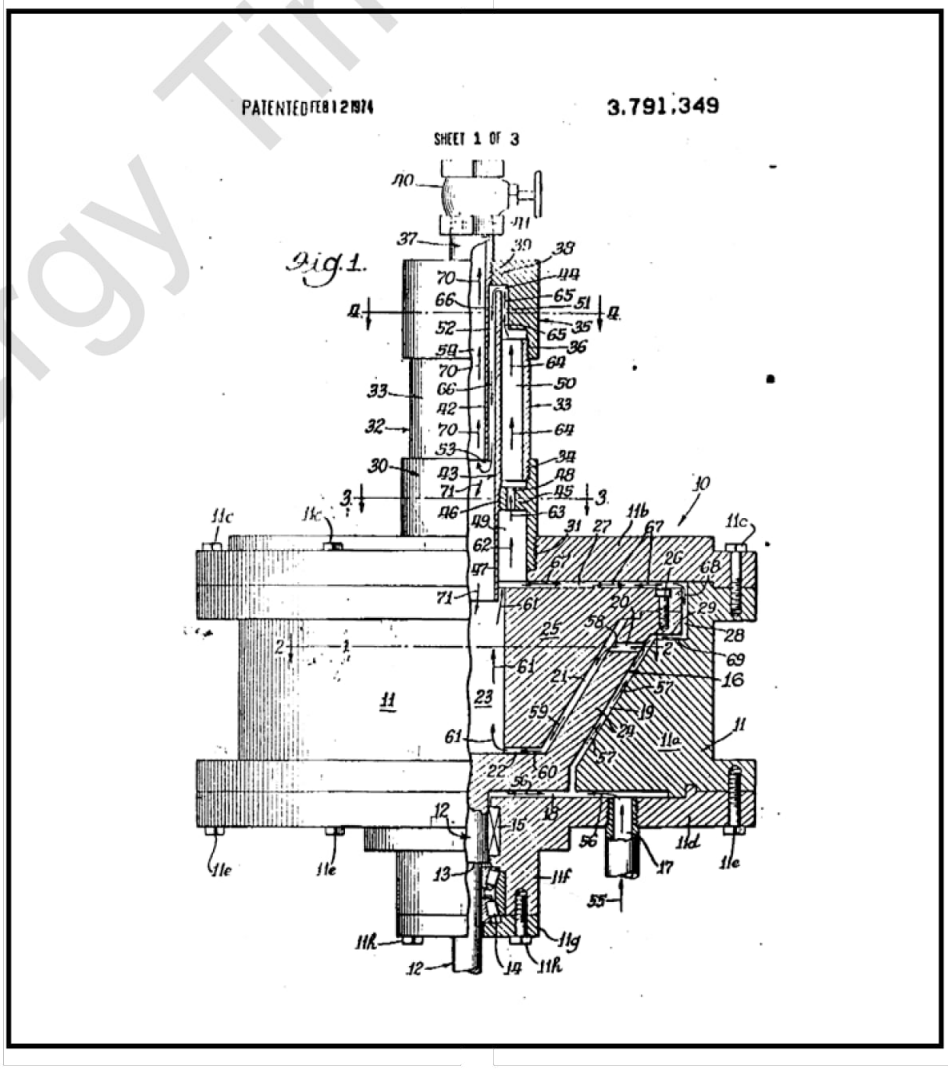
A separate test was made with immersion-type electric heaters. All engineers agree that this way of heating water is as close to 100% efficiency as you can get. Two 4.5 kilowatt immersion units were used in the same tank and through the same piping (two 4.5 KW immersion units = 30,708 B.T.U.H.). This 100% efficient water heater would not heat the house above 66 degrees F when the outside temperature was 15 degrees F. A fire had to be built in the fireplace to keep the house warm. The Aquasonic's Delta Tee unit did a better job for 10% less cost. I think a good way to test the B.T.U. output would be to build a 20' x 20' refrigerated cooler (like a meat cooler) and inside of this build a small house. This way we could have a controlled environment and measure the air temperature inside the house. In the past year we have tried almost every type of thermometer that is on the market and we can't believe the readings we get, nor could we get anyone else to believe them. With some of the readings we got, the unit showed as high as 700% efficiency. Several of the best engineers in the country tried to prove us wrong and couldn't. Many times the water temperature at the unit would read 150 degrees F., run through 100 ft. of 3/4" baseboard radiator and return to the unit at 152 degrees F, a gain of 2 degrees F after running through 100 ft. of radiator and giving off heat. This is impossible. It is also impossible to heat 2,880 sq. ft. of floor space with two 3-HP motors that equal 13,200 B.T.U.H., but the Aquasonic's Delta Tee does the job and does it well.

I think the Aquasonic's Delta Tee will help solve the energy problem and the polution problem and save on natural resourses.

# IDEAS:

- Buy bulk copies of *INFINITE ENERGY.*
- Give gift subscriptions to libraries and friends.
- Advertise in *INFINITE ENERGY.*
- Spread the message!

Below: Drawing from Schaefer's Steam Generator Patent #3,791,349

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## Cold Fusioneers Just Wan'a Have Fun!

The editor of this sedate journal recently purchased a "Cold Fusion Lives!" T-shirt from Al Kemme Associates—better to conduct serious editorial business in full cold fusion regalia! (He also purchased one for his son, Ethan, who had just turned 16 and was demanding a cold fusion-powered car for his birthday; he got an Alkemme T-shirt instead!) Packed in the package of T-shirts came the following letter from Al Kemme (Seriously —it *was* packed in the box!):

Al Kemme Associates
PO Box 503
Yellow Springs OH 45387-0503

Dear loyal customer:

Enclosed is your genuine Al Kemme T-Shirt. Sorry for the delay—thousands of orders to take care of, you know.

Now that you have your T-Shirt, you will certainly want to buy our other fantastic products:

1. *Al Kemme's Cold Fusion Coffee Maker*. uses excess heat to heat a mug of piping hot coffee. Saves 75% of the electricity required by old fashioned competing (non-nuclear coffee makers). Simply pour water into the machine (heavy water preferred, of course), add a little lithium hydroxide (results in a pleasing slightly salty taste), turn on the electrodes, wait two weeks for the excess heat to appear, and Bingo! You've got a cup of coffee! Amaze your friends! Only $97,500, guaranteed to produce excess heat.

2. *Al Kemme's Do-It-Yourself Hydrogen Bomb Kit.* The DOE would have you believe that you need all sorts of expensive equipment to make hydrogen bombs such as laser isotope enrichment plants. Hah! All you need is Al Kemme's deuterium glow discharge isotope generator, which transmutes ordinary elements to fissionable fuels for a fission charge, while also generating all the tritium you need for a fusion bomb. Why pay ridiculous prices on the black market when you can make what you need in the privacy of your own home? A real bargain at only $495,000 (export-restricted) .

3. *Al Kemme's Radioactive Waste Disposal System.* A more peaceful version of the Do-It-Yourself Hydrogen Bomb Kit, this uses the same deuterium glow discharge isotope generator to transmute radioactive materials into harmless stable isotopes. Why not buy one for the Department of Energy as a gift? Discounted at $445,000. Available for immediate export.

4. *Al Kemme Gasoline-Free Automobile.* We are pleased to announce that we have signed an exclusive agreement with Toyota to market the world's first cold fusion powered automobile, the Corolla-CF. Available 1998.

Well, thanks again for your order. Keep the Faith! Cold Fusion lives!!!

Best regards,
Al Kemme

# Recent Scientific Abstracts of Note

Obtained from the Los Alamos National Laboratory Preprint Archives:

**"Theory of quantum radiation observed as sonoluminescence"**
Claudia Eberlein, Department of Physics, University of Illinois at Urbana-Champaign, Urbana, IL 61801-3080 U.S.A. (Submitted June 7, 1995)

Sonoluminescence is explained in terms of quantum radiation by moving interfaces between media of different polarizability. In a stationary dielectric the zero-point fluctuations of the electromagnetic field excite virtual two-photon states which become real under perturbation due to motion of the dielectric.

The sonoluminescent bubble is modelled as an optically empty cavity in a homogeneous dielectric. The problem of the photon emission by a cavity of time-dependent radius is handled in a Hamiltonian formalism which is dealt with perturbatively up to first order in the velocity of the bubble surface over the speed of light. A parameter-dependence of the zero-order Hamiltonian in addition to the first-order perturbation calls for a new perturbative method combining standard perturbation theory with an adiabatic approximation.

In this way the transition amplitude from the vacuum into a two-photon state is obtained, and expressions for the single-photon spectrum and the total energy radiated during one ash are given both in full and in the short-wavelengths approximation when the bubble is larger than the wavelengths of the emitted light. A model profile is assumed for the time-dependence of the bubble during the collapse, and in this model the radiated energy and the spectrum are calculated numerically and in the short-wavelengths limit also analytically.

It is shown analytically that the spectral density has the same frequency-dependence as black-body radiation; this is purely an effect of correlated quantum fluctuations at zero temperature. The present theory clarifies a number of hitherto unsolved problems and suggests explanations for several more. Possible experiments that discriminate this from other theories of sonoluminescence are proposed.

**"Increased voltage phenomenon in a resonance circuit of unconventional magnetic configuration,"** *J. Appl. Phys.*, Vol.77(11), 1 June 1995, pp.6015-6020. Osamu Ide, Clean Energy Laboratory, Natural Group Corporation, Shinagawa, Japan

The behavior of an LCR (inductance-capacitance-resistance) circuit with a movable ferromagnetic core is discussed. The core is attracted by a magnetic field generated by an electric current resulting from the discharge of a capacitor in the closed LCR circuit. An unusual increase in recharge voltage, which was dependent on the magnetic configuration of the coil, was observed. This voltage increase does not conform to the mathematical simulation of the system. The possibility that a positive electromotive force was involved in this effect is discussed.

**"Maxwell's equations in a rotating medium: Is there a problem?"**, *Am. J. Phys.*, Vol.63(8), August 1995, pp.694-705. Gerald N. Pellegrini (Worcester, MA) and Arthur R. Swift, Dept. of Physics adn Astronomy, University of Massachusetts, Amherst, MA.

In 1908 Einstein and Lamb used special relativity to predict that a moving magnetic dipole devlops an electric dipole moment. The classic 1913 experiment of Wilson and Wilson on a polarizable, permeable medium rotating in an external magnetic field has long been cited as verifying this prediction. We argue that since the experiment involved rotation rather than uniform translation, it did not test special relativity. The analysis should properly be done in a rotating coordinate system. The field-equations for a rotating object are well known and the analysis is straightforward, but the result disagrees with the Wilson experiment. After carefully examining all steps in the derivation, we conclude that either the experiment is wrong or the theoretical analysis must be modified. One possible resolution of the conflict is the hypothesis that the dielectric constant $\varepsilon$ and the permeabiliy $\mu$ are well defined only in a frame in which the medium is at rest and time and space are orthogonal coordinates.

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# Mechanisms of a Disobedient Science : A Cold Fusion Theory

## How To Get Your Entropy in Order

by David Moon

**From its birth in 1989, cold fusion** has been a problem child, breaking all the rules. With a mind of his own, this new kid on the block has challenged nuclear experts with his chaotic behavior. He's also taught thinking people everywhere that we don't know it all, after all. And that is refreshing, at least to the open-minded visionaries who still understand the true nature of science.

But is there any measure of order in all this disorder? Perhaps there is. As a help, some cold fusion commentators have dusted off the writings of the medieval scholar William of Ockham (1285-1349) who penned: "Entities must not needlessly be multiplied." Known as "Ockham's razor" among scientists, E. F. Mallove (in *Fire from Ice*) has elaborated by stating: "Ockham's razor urges the initial choice of a simple explanation to encompass complex evidence, rather than a host of ad hoc theories. His theory-shaving device (hence the term razor) does not always work, of course, but it is worth thinking about when science is confronted with a bewildering array of seemingly contradictory experiments."

This bewildering array of experiments reads like a Disciplinary Report Card from our new kid's school: (1.) neutrons and tritium of the wrong energies and wrong numbers relative to each other and to the excess heat; (2.) helium-4 showing up when the heat arrives, but a little deficient (short on amount); (3.) gamma rays appearing to play hooky by hiding in the lattice (changing their vibrational modes).

And if that's not enough misbehavior, all this activity sometimes occurs when the kid is asleep, that is, under ambient conditions when no input power to the cold fusion cell is being supplied (e.g. Pons' and Fleischmann's "heat after death"). What to do?

In the spirit of Ockham, a comprehensive and global theory is going to be more satisfying to explain essentially all data in every kind of cold fusion experiment. Therefore, this model proposes to apply not only to the classical Pons - Fleischmann electrolysis, but also to molten salt electrolysis, proton conductors, $D_2$ gas loading, $D_2$ gas discharge, deuterium - ion bombardment, ultrasound - generated fusion, $H_2$ or $H_2O$ with nickel, and even the Hydrosonic Pump™ of James Griggs. The model of deuteron behavior described in this article is related to the known factors that define an active site - better termed a hyperactive site - on or near the metal surface. And the myriad of strange reactions should be allowed to occur without breaking many of the laws of physics.

One law that will be obeyed is the law of entropy. It might, however, be necessary to look at it from a different viewpoint in order to accommodate the new phenomena:

> Equalization of energy distribution, the driving force in the law of entropy, is theorized to be accomplished through coherency, not randomness.

In this new theory, chaotic cold fusion data are strangely tied to orderly behavior of deuterons concentrated in active volumes of the metal lattice. Of course, a lattice site will become active when certain conditions are met. These may be summarized as (a.) a special condition of matter (SCM) - a chemically assisted nuclear reaction - as described by Storms [1], and (b.) surface dynamics (SURFDYN) given by Glück [2]. which stresses the role of surface catalysis.

Peter Glück, in his SURFDYN hypothesis, has observed that, "The lack of reproducibility of the cold fusion experiments, aggravated by the great diversity and inconsistency of the positive results, implies that these nuclear phenomena are hypersensitive, i.e., correlated to a 'chaotic' factor", and that "Experimental data suggest that nuclear reactions take place in active sites on the surface of the lattice, that they are stimulated by dynamic factors..." [2.].

Some of these dynamic factors that stimulate the new effects are (a.) local concentration of deuterons, (b.) mobility [2] of the "plasmoidal deuterons" [3], (c.) temperature, electrical or other input into the cell, and (d.) the presence of stimulation effects such as magnetic fields and radio frequencies [4], as well as microwave application [5,6]. Nonequilibrium states are created.

The consequence of these dynamic factors in the concentrated active volume is to allow the nonequilibrium state to bring about collective, coherent oscillations of long trains (or chains) of deuterons against metal - atom barriers, such as a grain boundary. If the natural vibrations are in the microwave range, bathing the electrode or target metal with a resonant microwave frequency should enhance the reactions, which occur when trains of invading deuterons interact with the metal atoms in certain ways.

In fact, the effects should be greatest when the degree of coherency is greatest. Coherent microwave frequencies of the deuterated lattice that internally absorb fusion energy perhaps could be pumped up, in a maser-like fashion, resulting in a burst of microwave photons emitted from the cathode. Just such a burst might have been the cause of one unfortunate incident - a nontrivial burn suffered by an unidentified researcher when, it is believed, a burst of microwave energy from his cold fusion cell resonated with the atoms in his gold ring [7.]

Deuterons packed into an active volume will act collectively and coherently with metal atoms to instigate, at various stages of invasion into the metal, all the various chaotic reactions characteristic of most cold fusion experiments. Once understood, there is a good chance these reactions can be tamed and controlled.

The emphasis in this model, which may be called "Moon-on catalyzed fusion," is on a description of entropy, at least as it applies to deuterons stuffed into an active site. For deuterons that have filled the regular lattice spaces of palladium or titanium, coherent motion is believed to be the norm. These oscillating deuterons naturally will be at the lowest potential energy (relative to each other) only when moving together in the same direction. If the deuterons vibrate coherently between, say, boundaries of a grain defect or grain boundary, then the collective energy might become focused into a wave pulse which is able to penetrate the volume of a metal atom. Various reactions, outlined below, will have an opportunity to occur.

Therefore, it is postulated that

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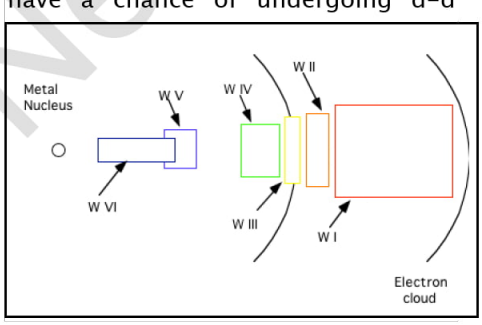
deuterons in the concentrated lattice do not gain greater disorder during equipartition of energy – at least not the kind of randomness that prohibits their coherent motion. Instead, the reality is an assembly of deuteron waves which tends toward coherency. Thus, coherent oscillations, even under lazy (ambient) conditions, can occur without help from the outside, which might explain why some cells won't shut off right away when the applied power is removed.

**Windows of Opportunity**

Finding the desired program on television is a two-step process: First turn on the TV set, then adjust the channel selector. In a cold fusion cell, step 1 involves creating an active site using principles given previously, principles painstakingly determined over five years by dedicated researchers. Step 2 is a matter of selecting the "input" (really, the extent of deuteron invasion into metal atoms). At various stages of deuteron invasion, different reactions are possible. Each stage is regarded as a window for a particular reaction to occur. A "window" is that region around a metal nucleus which, if entered by a line(train) of deuterons, can allow for a specific reaction..

Taken together, the various deuteron-metal interactions trace a "spectrum of reactions." This spectrum consists of window I – production of $^4$He with excess heat; window II – formation of "unstable" dineutrons, which produce some element or isotope transmutations (also tritium in window IV); window III – conventional fusion occurring through "partial" electron screening; window IV – manufacture of tritium; window V – formation of relatively stable dineutrons by electron capture, which are responsible for some neutron bursts (3-7 MeV in Pd) as well as fission of some metal nuclei; and window VI – generation of charged particles. The diagram below should be viewed as a general representation. Windows will overlap or be shifted slightly, depending on the metal atom involved:

**Window I** – Invading trains of deuterons that reach into window I have a chance of undergoing d-d fusion when the rebounding deuterons are compressed together with the help of electron screening. The electron cloud carries away the 23.85 MeV released, producing $^4$He. The probability for this heat – generating reaction should go up at (a.) higher local deuteron concentration, and (b.) at lower local input [8,9], that is, with less invasion into the metal atom.




Experiments that generate large amounts of excess heat might be producing a secondary fusion reaction, as well:

$$^4He + {}^2H \rightarrow {}^6Li + 1.47 \text{ MeV (lattice)}$$

In a manner similar to the reaction, $^2H + {}^2H \rightarrow {}^4He + 23.85$ MeV inside the electron cloud, the 1.47 MeV released in the $^4$He – deuterium fusion is connected to the dense electromagnetic field through multiple, opposite – spin virtual photons instead of being ejected as powerful gamma rays. Thus, the fusion energy "bleeds off" in smaller quanta, spreading instantly through a large domain of metal atoms that are acting as one entity. For example, if $10^9$ phonons (high – frequency vibrations of the lattice of metal atoms) are vibrating coherently, each at a frequency of $10^{12}$ Hz, then the total energy represents $10^{21}$ Hz, the order of frequency of a gamma emission.

Production of lithium-6 in some heat-forming cells could explain the shortage of $^4$He to account for all of the heat.

**Window II** – Deuterons reaching into the inner electron cloud of the metal atom will, of course, experience increasing Coulomb repulsive forces. The potential energy of the invading deuteron climbs – it experiences increasing excitation states. It has been observed by Jie Fu Yang, et al., (Hunan Normal University, China) that, "A deuteron has a weak force field to capture an electron if the deuteron is excited [10]". If the P.E. (deuteron) + E (orbital e$^-$) is resonant to a first excited state of a deuteron, the electroweak force would give capture of an electron on the deuteron a chance to occur in window II.

However, the total energy involved in the electron capture will be only enough to create a kind of "unstable dineutron," UDN (designated $^2n^*$). Evidence for the existence of such an intermediate dineutron is not known. It is postulated to form because it is a great convenience and will help explain a number of phenomena, including tritium formation in window IV. But meanwhile, in window II, UDN's might account for metal transmutations not tied to correlated neutron emissions. For example,

(a.) $^2H + e^- \xrightarrow{\text{e- cloud}} {}^2n^* + \text{neutrino}$

(b.) $^{108}Pd + {}^2n^* \rightarrow {}^{109}Pd + (p + e^-)$, Q = 3.9 MeV

(c.) $^{109}Pd \rightarrow {}^{109}Ag + \beta^- + \gamma + \text{neutrino}$

Step (b) shows the UDN half captured on Pd, where one of the neutrons in $^2n^*$ breaks up into a proton + electron as the fusion energy is released. Evidence for the energetic electrons would include bremsstrahlung emissions, whose energy might be UV to soft X-ray. Of course, the process of electron capture also will produce UV to X-ray radiation from orbital electrons in the metal dropping to lower quantum levels to replace the captured electron.

Isotopic transmutations that are due to the action of UDN's in window II are nearly neutronless and can occur side – by – side with the excess heat – forming reactions in window I. An occasional neutron may be detected, however, as the $^2n^*$ is half absorbed into the metal nucleus. The ejected neutron, instead of breaking into p + e$^-$, reabsorbs a quantum portion of the released energy to convert the p + e$^-$ combination into a neutron that can be counted in a neutron detector. A reasonable fractional estimation for this event is ~ $10^{-9}$ of the reactions between Pd nuclei and $^2n^*$. Therefore, it is easy to imagine window I occurring at a rate of 1 – watt output ($10^{11}$ – $10^{12}$ reactions/sec) while window II (believed to have less probability) occurs simultaneously at a rate of ~$10^9$ /sec, thus yielding ~ 1 n/sec.

In palladium systems the energy spectrum for these neutrons is estimated to be 2.5 – 6.5 MeV, from all Pd isotopes. (Neutrons at 3.0 – 7.0 MeV are emitted as a result of stable dineutrons forming in window V, below. In fact, neutron spectra containing 3 – 7 MeV bands have been observed by Dr. Akito Takahashi [8], as well as by others.)

At what energy level would UDN's form in the inner electron clouds? Only a very general estimate can be made. For a palladium atom, invading deuterons reach a potential energy of about 0.057 MeV at a distance of the first Bohr orbit, $r_1$ (quantum level n=1). This radius is calculated using the equation, $r = n^2 h^2 / 4\pi^2 m Z e^2$, and is judged to be the distance from the Pd nucleus where electron screening between deuteron and Pd nucleus has dropped to zero. So, the relation P.E. = $Z e^2 / r$ for the incoming deuteron is useable. For palladium atoms, $r_1 = 1.15 \times 10^{-10}$ cm.

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Since a value of less than 0.057 MeV will be estimated for the first excitation state of a deuteron, electron capture to make a UDN in Pd probably occurs before $r_1$ is reached by the invading deuterons. Still it is believed to occur somewhere in the inner electron cloud.

UDN's are predicted to be made also in titanium electrodes, where $r_1 = 2.4 \times 10^{-10}$ cm and P.E. of invading deuterons at $r_1 = 0.013$ MeV, a value probably closer to that needed to convert a deuteron to $^2n^*$. However, only Ti-50 will show an element change:

$$^{50}\text{Ti} + {}^2n^* \rightarrow {}^{51}\text{Ti} + (p + e^-)\text{, then}$$

$$^{51}\text{Ti} \rightarrow {}^{51}\text{V} + \beta^- + \gamma + \text{neutrino, half-life} = 6 \text{ min.}$$

UDN's in titanium, as in palladium, will be used in window IV to make low energy tritium.

**Window II As Heat Source –** Reactions between UDN's and metal nuclei in window II that happen to suddenly occur at elevated rates, i.e. $\geq 10^{12}$/sec instead of the estimated $\leq 10^9$/sec, should become a source of excess heat. At these increased rates the ratio W II / W I might become $\geq$ 1/1, instead of the usual $\leq 10^{-3}$. If so, neutron emissions will climb from rates on the order of ~1n/sec to $\geq 10^3$ n/sec (where n/reaction $\approx 10^{-9}$). But X-ray emissions correlated to UDN activity in window II also increase to $\geq 10^{12}$/sec in these examples, and perhaps is the culprit to explain various "X-ray incidents" that have been reported.

In addition, element transmutations and isotope shifts become abundant, even to the point of accounting for all of the excess heat in some experiments. Such correlations have been measured by Chuan-Zan and Yi-Fang (China) and Bazhutov, et al. (Moscow),[11].

**Window III –** Evidence for conventional d + d fusion does exist because of the appearance of 2.45-MeV neutrons from the helium-3 branch and 3-MeV protons from the tritium branch in some experiments. Trains of invading deuterons that reach into window III will, upon recoil from the Coulomb force, experience partial electron screening - enough to allow for d + d fusion (if sufficiently close together), but not enough electron density to carry away much of the fusion energy. Therefore, in the helium-3 branch, neutrons with energies 2.45 MeV are expected. In fact, it was reported at the Nagoya Conference that a Chinese research team found that their "neutron spectrum contained both 2.0-2.5 MeV neutrons, and broad emission between 2.5-7.0 MeV; most of the emissions occurred above 2.5 MeV", [12].

Window III is only a porthole - a narrow window at the inner edge of the metal atom's electron cloud. Invading deuteron trains can easily miss the window, explaining the very hypersensitive nature of producing 2.45-MeV neutrons or 3-MeV protons within this spectrum of reactions. An active volume that is "tuned" to window III momentarily would be capable only of a burst of neutrons on occasion. Of course, conventional d + d fusions outside of the "spectrum of reactions" are possible in any active cell experiencing intense local agitation.

The placement of window III near window IV (tritium production) suggests that chaotic neutron bursts during tritium formation are not completely unexpected. Such sporadic nuclear behavior is inherently part of the personality profile of cold fusion phenomena - and of this model. On the other hand, to use the words of Glück [2], the "hyperfine structure" of these phenomena means that tritium possibly can appear in window IV, while window III (2.45-MeV neutron) is bypassed altogether.

**Window IV –** Tritium is known to form at very low energy, energy too low to effect a secondary tritium-deuteron fusion, which yields a 14-MeV neutron[1]. Moreover, the appearance of cold fusion's strange tritium is accompanied by a wide - ranging neutron/tritium ratio of $\sim 10^{-9}$ to $10^{-3}$ [13], still far too low to be attributed to conventional d +d fusion which yields a ratio of about 1 n/1 tritium.

Tritium manufacture is proposed to be a two-step process:

(1.) The leading deuteron of an invading train of deuterons reaches just beyond the inner electron shell of the metal atom in window IV. Meanwhile, a trailing deuteron weakly captures an orbital electron to form a UDN;

$$d + e^- \rightarrow {}^2n^* + \text{neutrino}$$

(2.) The leading deuteron rebounds back to the UDN;

$$d + {}^2n^* \rightarrow T + (p + e^-),\ Q \approx 4 \text{ MeV}$$

Most of the fusion energy is carried away by one of the neutrons in the dineutron, which immediately disintegrates into a proton + electron. Supporting evidence for this mechanism would be the detection of low-level radiation produced by thermalization of the fast electrons generated during tritium formation, as well as UV and X-ray emissions from Pd (or Ti) as UDN's form.

Two advantages of this tritium mechanism are (1.) accomplishment of making low-energy tritium, and (2.) tritium appearing with few or no neutrons. The maximum n/T ratio expected in window IV might be $\sim 10^{-9}$, when a quantum portion of the fusion energy in the d + $^2n^*$ reaction is reabsorbed to produce a neutron instead of the p + e- combination. But since the n/T ratio can climb to $10^{-3}$ in some experiments, neutrons from window III likely accompany tritium production at times. The narrowness of the windows could explain the burst-like nature of these events. (In addition, neutrons from window V might accompany tritium formation when oscillating trains of deuterons experience maximum resonance with their own reflected waves, resulting in further deuteron invasion into metal atoms.)

Heat is sometimes detected during the production of tritium and neutrons; sometimes it is not [6.]. This would suggest some local conditions in an active volume might favor windows III and IV, but not window I. Or in other experiments, only window I is promoted.

In the ultrasound experiments at E-Quest Sciences (Palo Alto, CA) it appears that window I dominates. Roger Stringham and Russ George have produced remarkable levels of continuous excess heating - 90 watts or more for several days - using only the input of acoustical energy. Although they get isotopic transmutations, neutrons and tritium are absent [7.]

During the action of cavitation on the palladium surface, which is generated by the acoustic waves passing through the heavy water, this new theory would propose that collective, coherent oscillations of deuterons against metal atoms occur by virtue of their own thermal vibrations. Invasion into window I, and perhaps II, is accomplished, but not further. Plenty of $^4$He is made, but it usually completes only 20% of the needed bookkeeping for the excess heat[7].

However, it might be true that $^4$He production actually supplies as much as 80-90% of the heat generated in their ultrasound (Micro Fusion) cells, and that ~90% of the helium made is

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converted to $^6$Li in a sequential fusion with another deuteron in a train of deuterons: $^4He + {}^2H \rightarrow {}^6Li$ + 1.47 MeV (lattice dispersed). At a rate of 90 watts for three days lithium-6 on the order of $10^{18}$ atoms would accumulate. In at least one experiment at E-Quest Sciences, tests showed the presence of lithium. But it was understandably dismissed as a contaminant in the testing apparatus[7].

One very interesting thing does occur in test cells at E-Quest Sciences. They discovered they can consume tritium[7]. Therefore, as others who do produce tritium sometimes report a decline in tritium activity as electrolysis continues[14], the following reactions are postulated:

$$t + d \xrightarrow{\text{e-CLOUD}}$$

$$^4He\ (<\text{thermal}) + 17.6\ \text{MeV (lattice)} + n\ (<\text{thermal})$$

followed by (example):

$$n(<\text{thermal}) + {}^{105}Pd \longrightarrow {}^{105m}Rh + p(\sim 88\ \text{KeV})$$
$$\hookrightarrow {}^{105}Rh + g(129\ \text{KeV})$$

It is known that the 129-KeV line in a gamma spectrum was identified by Dr. Yan Kucherov in gas-discharge experiments. A measured half life of 45 seconds would indicate metastable rhodium-105 as the likely source of this gamma line, but admittedly does not guarantee the above reaction as the source of the rhodium.

**Window V** - More neutron bursts are proposed to be possible when the lead deuteron invades the metal atom into window V. Here a deuteron captures an inner-orbital electron to become, in relative terms, a stable dineutron, or SDN (denoted $^2n$). These dineutrons, in addition to producing free single neutrons when half captured on palladium (or just about any metal nucleus), will sometimes cause fission reactions when completely captured by heavier metal nuclei.

SDN's differ from UDN's (formed in window II) in the sense that more energy is put into the deuteron when an inner orbital electron is captured to make an SDN. That is, the deuteron has reached further into the metal atom and has climbed to a higher potential energy within the nuclear Coulomb field. If the excited deuteron captures an orbital electron at this point, the P.E. (deuteron) + E (orbital $e^-$) is probably resonant to a second excited state of the deuteron. An electron is taken from the inner electron cloud of the metal atom to form an SDN.

The late Nobel Laureate physicist Julian Schwinger, in his advanced theory, has helped many who ponder cold fusion mysteries to make the paradigm shift from hot-fusion to cold-fusion thinking. In a paper presented at the ICCF-4 in Maui (read for him in his absence by Eugene Mallove), Dr. Schwinger states: "...in very low energy cold fusion one deals essentially with a single state, or wave function, all parts of which are coherent. It is not possible to totally isolate the effect of the electric forces from that of the nuclear forces. The correct treatment of cold fusion will be free of the collision-dominated mentality of the hot fusioneers" [15]. Thus, capture of an orbital electron by invading deuterons must be the result of a more expressive electroweak force which is responsible for converting a deuteron to a dineutron in window V.

To estimate the energy for this transition from deuteron to SDN, it may be allowable to begin with energies of the emitted neutrons. Neutron energy spectra from palladium include a broad band from 3-7 MeV. If the value E = 0.51 MeV is used in the reaction $d + e^- + E \rightarrow {}^2n$ + neutrino, the mass of $^2n$ becomes 2.014649 amu. Using this mass, maximum and minimum neutron energies are:

$$^{105}Pd + {}^2n \rightarrow {}^{106}Pd\ (0.04\text{MeV}) + n\ (7.01\ \text{MeV})$$
$$^{110}Pd + {}^2n \rightarrow {}^{111m}Pd\ (0.04\text{MeV}) + n\ (3.03\ \text{MeV})$$

In a very general way window V could be placed inside a metal atom in the region where the potential energy on the invading deuteron plus the energy, $E(e^-)$, carried by the captured electron (from n = 1) totals a resonant value of 0.51 MeV. That is, 0.51 MeV = P. E. (deuteron) + $E(e^-)$, where $E(e^-)$ = initial K. E. (at $r_1$) + $\Delta$ P. E. ($r_1$ to r), and where $\Delta$ P. E. ($r_1$ to r) = $-Ze^2/r_1 - (-Ze^2/r)$, relative to Pd nucleus.

For example, in palladium atoms (Z = 46), P.E. (deuteron) = 269.3 KeV at r = 0.246 x $10^{-10}$ cm, (where $r_1$ = 1.15 x $10^{-10}$ cm), and $E(e^-)$ = 28.8 KeV - 57.6 KeV - ( - 269.3 KeV) = 240.5 KeV. Thus, total E = 269.3 + 240.5 ≈ 510 KeV.

For deuteron invasion into titanium, P. E. (deuteron) = 259 KeV at r = 0.122 x $10^{-10}$ cm, (where $r_1$ = 2.4 x $10^{-10}$ cm), and $E(e^-)$ = 6.6 KeV - 13.2 KeV - (-259 KeV) = 252.4 KeV. Therefore, total E = 259 + 252 ≈ 511 KeV.

In platinum, P. E. (deuteron) = 296 KeV at r = 0.38 x 10-10 cm, (where r1 = 0.68 x 10-10 cm), and E(e-) = 82.5 KeV - 165 KeV - (-296 KeV) = 213.5 KeV. Thus, total E = 296 + 213.5 = 509.5 KeV. As an impurity in palladium, platinum might help write some of the nuclear signatures, as well. Of course, since Z = 78, an incoming deuteron reaches the resonant excitation state much sooner than in Pd or Ti - closer to the inner electron cloud. Perhaps this increases the chance for creation of SDN's in platinum atoms. If so, the presence of large atoms such as Pt and Au at the active site (especially as Au coating) is predicted by this theory to show increased probability for neutron emissions, and could account for some of the larger bursts (e.g. $10^6$ n/s) that have been reported.

What is the lifestyle of a stable dineutron? Two categories of interaction with metal nuclei are assumed: (1.) The SDN is half captured by a metal nucleus to form a free neutron and a new isotope. This is the ($^2$n,n) reaction. (2.) An SDN is fully captured by a metal nucleus, causing either (a.) symmetric or asymmetric bifission, (b.) super asymmetric fission[16], or (c.) alpha-particle ejection.

Table 1 shows the Q-values for $^AM$ $(^2n,n)^{A+1}$ M reactions applied to various metals in cold fusion cathodes. When SDN's form in window V they have a certain probability of producing single-neutron events or small or large neutron bursts. The spectrum of neutron energies for each metal should have peaks that match isotopes having (a.) the largest percent abundance, and (b.) the largest neutron-absorption cross sections.

**Table 1** - Neutron spectra for $^AM$ $(^2n,n)^{A+1}$ M reactions for various metals (host, impurity or coating) in cold fusion electrodes. The list is a sampling.

| Metal | Estimated Neutron Energies(MeV) |
|---|---|
| Pd | 3.03 - 7.02 |
| Ag | 4.35 - 4.75 |
| Cd | 3.25 - 6.85* |
| Pt | 3.05 - 5.45 |
| Au | 3.95 ** |
| Mn | 4.75 |
| Ti | 3.85 - 9.05 |

* At 20,000 barns for its n-absorption cross section, Cd-113 (12% of Cd) will dominate, producing ~6.5 MeV neutrons. However, this very high neutron-capture cross section might intercept the formation of SDN's, and instead react immediately with an incoming deuteron to form energetic protons (See window VI).

**Au-198 that forms has a neutron-absorption of its own equal to 26,000 barns; so it could show high ($^2$n,n) activity, or charged-particle activity.

Besides producing single or bunched neutron emissions, the ($^2$n,n) reac-

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tions leave behind transmuted elements and shifted isotopes. In fact, evidence has been mounting that such nuclide changes in cold fusion cathodes are extensive. Professors J. O'M. Bockris and R. Sundaresan (Texas A & M University) gave a summary of these findings, most of which were presented at ICCF-4 in Hawaii, in their paper at the Minsk Conference[11]. New elements mentioned include Rh, Ag, Cd-114, In, Sn, Te-130, Xe-136, W-184, Os-192, Au, Sr, Zr, Nb and Mo-100. Researchers claim many other elements have appeared in addition to these.

Sample reactions in window V that might account for these element and isotope changes are given below. They cover not only the ($^2$n, n) reactions but also various fission reactions which SDN's might cause when they become totally absorbed into the metal nucleus:

1. $^{A}M(2n,n)\,^{A+1}M$ Reactions.

(a.) $^{108}_{46}Pd + {}^2n \rightarrow {}^{109m}_{46}Pd + n$ (Q = 3.46 MeV)

followed by ,

$^{109m}_{46}Pd \xrightarrow{(4.7m)} {}^{109}_{46}Pd + \gamma\,(188\,KeV)$

$\xrightarrow{(13.5\,h)} {}^{109}_{47}Ag + b^- + g + neutrin$

(b.) $^{196}_{78}Pt + {}^2n \rightarrow {}^{197m}_{78}Pt + n$ (Q = 3.07 MeV)

followed by,

$^{197m}_{78}Pt \xrightarrow{(78m)} {}^{197}_{78}Pt + \gamma\,(279\,KeV)$

$\rightarrow {}^{197}_{79}Au + b^- + g + neutrin$

$^{50}_{22}Ti + {}^2n \rightarrow {}^{51}_{22}Ti + n$ (c.)

$\xrightarrow{(5.8\,m)} {}^{51}_{23}V + b^- + g + neutrin$

perhaps followed by,

$^{51}_{23}V + {}^2n \longrightarrow {}^{52}_{23}V + n$

$\xrightarrow{(3.7\,m)} {}^{52}_{24}Cr + b^-\,(2.47\,MeV) + g\,(1.43\,MeV) + neutrin$

2. Symmetric Bifission.

(a.) $^{104}_{46}Pd + {}^2n \rightarrow {}^{58}_{26}Fe + {}^{48}_{20}Ca$

(b.) $^{192}_{78}Pt + {}^2n \rightarrow {}^{56}_{24}Cr + {}^{136}_{54}Xe + 2n$

$\xrightarrow[(6m)]{-b^-,g} {}^{56}_{25}Mn \xrightarrow[(2.5\,h)]{-b^-,g} {}^{56}_{26}Fe$

3. Superasymmetric Fission.

(a.) $^{102}_{46}Pd + {}^2n \rightarrow {}^{87m}_{38}Sr + {}^{17}_{8}O$

$\xrightarrow{(2.8\,h)} {}^{87}_{38}Sr + g\,(388\,Ke)$

(b.) $^{196}_{78}Pt + {}^2n \rightarrow {}^{184}_{72}Hf + {}^{14}_{6}C$

$\xrightarrow{-b^-,g} {}^{184}_{73}Ta \xrightarrow[(8.7\,h)]{-b^-,g} {}^{184}_{74}W$

4. Alpha - Particle Emission.

(a.) $^{48}_{22}Ti + {}^2n \rightarrow {}^{46}_{20}Ca + {}^4_2He$ (Q = 5.87 MeV)

(b.) $^{104}_{46}Pd + {}^2n \rightarrow {}^{102}_{44}Ru + {}^4_2He$ (Q = 109 MeV)

(c.) $^{194}_{78}Pt + {}^2n \rightarrow {}^{192}_{76}Os + {}^4_2He$ (Q = 12.34 MeV)

5. **Special Case: Cadmium-113**. When nuclear scientists and engineers want to employ a material that strongly absorbs neutrons, as in nuclear-fuel control rods, they often hire a cadmium-113 atom (or similar atoms like boron). At 20,000 barns for its thermal-neutron absorption cross section, Cd-113 (which is 12% of Cd) that is alloyed with Pd or Ti is likely to make its presence known by easily reacting with approaching deuterons.

A deuteron is a neutron-carrying proton as far as some cold fusion reactions are concerned. In a Cd-113 atom, the radius of the neutron-capture area(0.8 x $10^{-10}$ cm) extends almost out to the first Bohr orbit (1.1 x $10^{-10}$ cm). Invading deuterons might need to approach to within about 0.25 x $10^{-10}$ cm of the cadmium nucleus where P.E. (deuteron) + E(captured $e^-$)= 510 KeV before they can capture an orbital electron to form an SDN. Thus, SDN formation is easily thwarted - the incoming deuteron quickly loses its neutron to the 113Cd nucleus, while having its proton rejected at ~ 6.8 MeV (see footnote in Table 1).

However, if while passing through window II, the deuteron converts to a UDN ($^2$n*), the outcome could be different. A UDN approaching the Cd-113 nucleus is predicted to initiate the following reactions:

$^{113}_{48}Cd + {}^2n^* \xrightarrow{(-e)} {}^{115m}_{49}In + \gamma,$ (Q= 13.3 MeV

$\xrightarrow{(4.5\,h)} {}^{115}_{49}In + g\,(335\,Ke)$

followed by,

$^{115m}_{49}In + {}^2n \rightarrow {}^{116m_2}_{49}In + n$

and,

$^{115}_{49}In + {}^2n \rightarrow {}^{116m_2}_{49}In + n$

then,

$^{116m_2}_{49}In \xrightarrow{(2s)} {}^{116m_1}_{49}In + \gamma\,(164\,KeV$

$^{116m_1}_{49}In \xrightarrow{(54m)} {}^{116}_{49}In + \gamma$

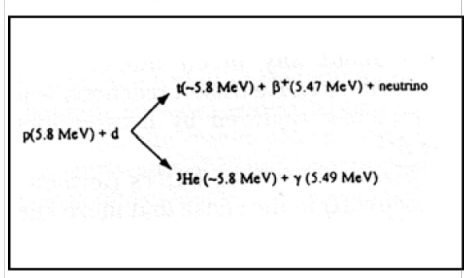
$^{116}_{49}In \xrightarrow{(13s)} {}^{116}_{50}Sn + \beta^- + \gamma + neutrin$

Both indium and tin are listed by Y. Kucherov among the new elements detected in cold fusion experiments[11]. Small amounts of cadmium impurities in palladium might be the starting point for the synthesis of these two new elements.

**Window VI** - Here production of charged particles by donation of the neutron from deuteron to metal nucleus occurs, kicking back the proton at MeV energy. In the invading trains of deuterons, sequential fusions will be possible. For example, in titanium systems there is evidence for the following scenario:

$d + {}^{48}Ti \rightarrow {}^{49}Ti\,(0.12\,MeV) + p\,(5.80\,MeV)$

The energy distribution of the Q-value (5.92 MeV) is determined using classical momenta calculations. The 5.8 MeV proton could immediately react with a trailing deuteron during deuteron invasion:




The value of 5.8 MeV is a median energy. The triton can range from 5.45 MeV (positron + neutrino ejected straight forward) to 6.15 MeV (when ejected straight backward). Nearly the same energy range occurs for $^3$He.

Numerous interesting experiments measuring energy spectra of charged particles have been conducted. Among them are the experiments by Dr. George Chambers, et al., at the U.S. Naval Research Laboratory[17].

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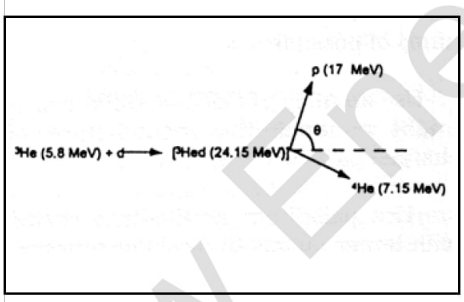
The NRL scientists bombarded a thin titanium film with 350-eV deuterium ions. Charged particles at an energy peak of 4.99 MeV were collected in one experiment. The researchers estimated that this peak was mostly accounted for by tritons generated at a starting kinetic energy of 5.38-6.0 MeV [17].

A more recent report of 17-MeV protons, observed coming out of highly deuterated Ti $D_X$ during bombardment with 150-KeV deuterons [18], suggests further sequential fusions are possible. The researchers, Jirhota Kasagi and Keizo Ishii (Tohoku University) found that when the loading ratio (the number x in Ti $D_x$) exceeded about 1.2, energetic protons were "grouped at energies around a 14.1 MeV peak, with a range from 12.5 to 16.5 and extending to around 17 MeV"[19].

At the higher deuteron concentrations, and with the kind of coherent, resonating deuteron oscillations needed for maximum penetration into the volume of a metal atom, additional sequential fusions become possible. For example, $^3$He particles can react with another trailing deuteron:

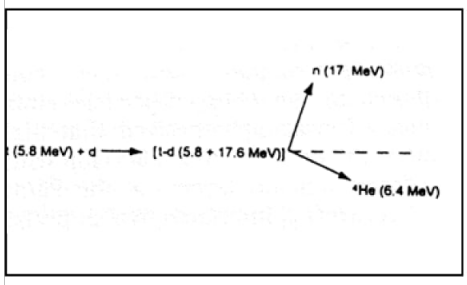
$$^3\text{He} + \text{d} \rightarrow {}^4\text{He (3.69 MeV)} + \text{p(14.66 MeV)}$$

The Q-value (3.69 + 14.66 = 18.35 MeV) can be distributed in a way that will produce a 17-MeV proton, where the total kinetic energy in this example is ~ 5.8 + 18.35 ≈ 24.15 MeV. This should occur when q ≈ 66°.




Generally, if the proton energies in Dr. Kasagi's experiments range from 12.5 to 17 MeV, the other fragment - the $^4$He - should range from about 7-11.5 MeV. Akito Takahashi (Osaka University) saw an 8-MeV helium peak when he implanted titanium with a deuteron beam [19,20]. Perhaps the same reactions could explain his findings.

The other branch of the p(5.8 MeV) + d fusion in titanium gives tritons at a median 5.8 MeV. These should be able to generate an occasional 17-MeV neutron:




Neutron spectra with energies up to 17 MeV were measured by Dr. Yan Kucherov, et al., in deuterium gas-discharge experiments using palladium cathodes[21]. Palladium also gives charged-particle emissions from deuterium-ion bombardment. Two of the many consistent findings were reported by Drs. E. Cecil, Colorado School of Mines [22] and A. Takahashi [20]. They both found energy peaks around 5 MeV.

In the titanium experiments, preference was given to Ti-48 rather than to isotopes 46, 47, 49 or 50. That's because $^{48}$Ti has the greatest abundance (74%) and the largest thermal-neutron cross section at 8 barns. This is not assuming that there is a direct correlation between n-absorption areas and the strength of reaction between a deuteron and metal nucleus in forming charged particles. (It has not been shown by experiment, but a good test would be to add a strong neutron-absorber to the cathode such as cadmium.) Even if the reaction, $^A$M + d → $^{A+1}$M + p(MeV), turns out to be a resonant effect, the deciding factor determining which nuclides react could be related to the threshold energy required to propel the oscillating wave-pulse of deuterons to within a certain distance from the metal nucleus. (For $^{48}$Ti the threshold energy needed to enter the neutron-absorption area is 1.98 MeV, which is found from the relation,
$E = Z e^2/d$.)

This seems like a Coulomb-barrier problem, indeed. But in the new field of solid-state physics known as Cold Fusion, there is theorized to be a tremendous focusing of energies made possible in deuterated lattices, brought about by a unique organization of matter (coherent waves), that defy the conventional view of entropy as disorder, which would break down coherent states. To visualize individual deuterons-or even groups of deuterons- invading close to a single metal nucleus to instigate a reaction is difficult to do.

Dr. Martin Fleischmann has provided an insightful view: "In quantum field theory you state from the beginning that you have many, many species. If we try to understand the behavior of a fixed number of atoms that are supposed to be isolated from their surroundings, then we have bought ourselves a great deal of trouble. If instead we wish to prove a certain piece of a large mass, then there are no difficulties whatsoever with the measurement problem,"[23].

And related to this, Dr. Francesco Scaramuzzi (Frascati National Laboratory, Italy) says, "there is a connection between cold fusion and superconductivity since they are both substantially collective phenomena. You can measure properties of a mass of material which are explainable only in terms of quantum mechanics, but on the macro scale. You don't have to look at single nuclei"[23].

Is the production of charged particles a resonant effect between incoming trains of deuterons and the metal nucleus? Recalling the study of uranium, theoretical work by Niels Bohr and John A. Wheeler showed that $^{235}$U underwent fission when absorbing slow or thermal neutrons, and not the more abundant $^{238}$U (except to a slight extent). It was also discovered that $^{238}$U exhibits a strong increase in neutron absorption if the neutron is moving with an energy of about 25 electron volts (1000 x thermal). This is called resonance capture, of course, and occurs only if the neutron is moving at a very definite speed. Trains of deuterons approaching a metal nucleus will pass through a range of kinetic energies from > thermal to thermal to < thermal. Perhaps neutron transfer from deuteron to metal nucleus occurs at a specific energy during invasion.

Whether the reactions are related to a resonant effect or a thermal neutron-capture effect, the data from charged-particle emissions seem to show that a select group of metal nuclides are responsible for these reactions and that charged-particle energies are not due to the whole range of isotopes in a metal. For palladium systems, an energy peak close to 5 MeV might be a clue about which metal isotopes are involved.

Among the palladium isotopes, $^{108}$Pd has the largest thermal-neutron absorption cross section at 12 barns.

The primary reaction is: d + $^{108}$Pd → $^{109}$Pd (0.04 MeV) + p(3.89 MeV).

The threshold energy needed for deuteron invasion into the n-absorption area, if this close approach is nec-

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essary, is 3.38 MeV. (In this theory, such focused energy is provided by the collective, coherent deuteron oscillations in the form of a resonating wave pulse in the active volume.) The kinetic energy of the primary protons generated by reacting $^{108}Pd$ + d is 3.89 MeV.

Therefore, it is more reasonable to predict that active impurities in palladium or certain alloying or coating material having lower threshold energies for reaction are responsible for generating charged particles of particular energies.

Table 2 lists a few of these metals and the energy range of charged particles each could produce.

Palladium samples containing silver impurities, for example, might witness the following typical reactions when local conditions in the active site encourage charged-particle formation in window VI:

Table 2 - Energy range of primary protons, secondary tritons and $^3He$, and tertiary alpha particles.

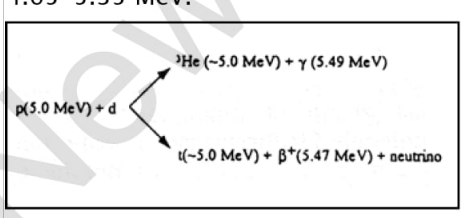
| Isotope | Neutron absorption cross section (barns) | Threshold energy to invade neutron-absorption area (MeV) | Energy of priimary proton (MeV) | Energy range of t or $^3He$* | Approximate median energy of $^4He$ when accompanying p or n = 15MeV* |
|---|---|---|---|---|---|
| $^{48}Ti$ | 8 | 1.98 | 5.8 | 5.45 - 6.15 | 8.05 - 9.5 |
| $^{55}Mn$ | 13.3 | 1.74 | 4.95 | 4.6 - 5.3 | 7.2 - 8.65 |
| $^{108}Pd$ | 12 | 3.38 | 3.9 | 3.55 - 4.25 | 6.15 - 7.6 |
| $^{107}Ag$ | 35 | 2.05 | 5.0 | 4.65 - 5.35 | 7.25- 8.7 |
| $^{109}Ag$ | 89 | 1.27 | 4.55 | 4.2 - 4.9 | 6.8 - 8.25 |
| $^{113}Cd$ | 20,000 | 0.086 | 6.75 | 6.4 - 7.1 | 9.0 - 10.45 |
| $^{195}Pt$ | 29 | 3.69 | 5.65 | 5.3 - 6.0 | 7.9 - 9.35 |
| $^{197}Au$ | 98.8 | 2.03 | 4.25 | 3.9 - 4.6 | 6.5 - 7.95 |
| $^{198}Au$ | 26,000 | 0.12 | 5.3 | 4.95 - 5.65 | 7.55 - 9.0 |

* Assumes immediate sequential fusion (no energy loss).

<u>Primary protons</u> -

$$d + {}^{107}Ag \rightarrow {}^{108}Ag\ (0.05\ MeV) + p\ (5.00\ MeV)$$

<u>Secondary $^3He$ or tritons</u> - approximate range of $^3He$ and t = 4.65-5.35 MeV.




**<u>Tertiary 4He, protons</u>** - will form sequentially at higher D/Pd (i.e. 1.2, Kasagi); sample energy distribution is:

$$^3He\ (\sim 5.0\ MeV) + d \rightarrow {}^4He\ (8.7\ MeV) + p(14.65\ MeV)$$

$$t\ (\sim 5.0\ MeV) + d \rightarrow {}^4He(8.5\ MeV) + n\ (14.1\ MeV)$$

A very interesting set of charged-particle data was generated at NTT Laboratories in Tokyo by Drs. Eiichi Yamaguchi and Takashi Nishioka [24]. Their unique electrode design of a 1-mm palladium foil sandwiched on one side by a gold layer and the other side by manganese oxide might have contributed to the charged-particle emissions. It was hypothesized that high deuterium concentrations develop within "accumulation layers" at the Pd/Au and $Pd/MnO_{(x)}$ interfaces, which probably creates much higher local loading ratios than the overall 0.6 D/Pd measured.

Energetic charged particles that form at the gold layer, however, would not be detected since 5-MeV particles could not pass through 1 mm of Pd foil to reach the detector, which was placed 6 cm in front of the manganese oxide surface. Most of the energy peaks were in the 4.5-6.0 MeV range. Based on energy shifts using an intervening 7-mm Al foil, these particles were judged to be either alpha particles or $^3He$ nuclei.

Mn-55 could produce primary protons and secondary $^3He$ at a median 4.95 MeV, and tertiary $^4He$ at a median 8.6 MeV, (when its accompanying proton is 14.7 MeV). And as reviewed above, silver gives very similar energies. Dr. Yamaguchi's data showed an alpha-particle peak at 8.5 or 8.6 MeV, but their apparatus was not set up to measure protons above 10 MeV.

Finally, a mysterious grouping of signals appeared at 6.6 - 7.0 MeV. Perhaps cadmium was present:

$$d + {}^{113}Cd \rightarrow {}^{114}Cd\ (0.06\ MeV) + p\ (6.76\ MeV),$$

$$p\ (6.76\ MeV) + d \rightarrow {}^3He\ (med.\ 6.76\ MeV) + \gamma\ (5.49\ MeV)$$

where maximum range of $^3He$ = 6.4 - 7.1 MeV.

Production of charged particles has been presented as occurring during a straightforward invasion of trains of deuterons, denting the metal atom's volume all the way into window VI. The invasion energy is supplied by a resonating deuteron wave pulse brought about by a high voltage input and / or incidental application of electromagnetic radiation during deuterium-ion implantation or gas-discharge experiments. But some researchers have reported that charged-particle signals sometimes continue after the deuteron beam is turned off. Iida and colleagues (Osaka, Japan) observed a 5-MeV emission from palladium this way [1]. M. I. Martinov, et al., (Kurchatov Inst.), targeting titanium, saw persistent signals in their detector for hours after the beam was switched off [25].

Admittedly it is a paradigm shift, but this cold fusion theory takes the general view that highly coherent, resonating deuteron oscillations can occur within highly concentrated active sites. The available energy is amplified and focused, even during lazy (ambient) conditions of the deuterated lattice, to make possible any of the reactions in windows I - VI, as long as the environment for an active volume exists [1,2,3]. In the event invasion of deuterons all the way into window VI under ambient conditions is too remote, an alternative mechanism to generate charged particles can be considered. An example is:

**Opening the Windows**

Previously this author suggested two kinds of possible test cells [26]:

$$^2n^* + {}^{10}Pd \xrightarrow{-e^-} {}^{10}Pd\ (0.04\ MeV) + p(4.83\ MeV)$$

1. Use an alloy of CdPd or GdPd which might enhance the manufacture of charged particles in window VI.

2. Use palladium or titanium coated with larger metals like gold or tungsten to increase electron screening. The idea is to promote the heat-forming reactions in window I. A microwave flux of certain frequencies and intensities would be applied to the cathode.

At first, alloys of cadmium or

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gadolinium with Pd or Ti had interest only as test cathodes. That is because charged particles generally do not show a connection to excess heat. However, this thinking might be wrong. A preliminary test using a CdPd alloy could be conducted (carefully). If the test gives a strongly positive result, practical applications become imaginable. Because of huge neutron-absorption areas for Cd and Gd, very high reaction rates with deuterons in window VI are expected. Sequential fusions perhaps would take place inside the metal's electron cloud. If true, it might reduce secondary radiation effects expected from high rates of charged-particle production, and lead to a proliferation of excess heat.

One drawback is obvious. Once reacted, an active isotope of cadmium or gadolinium is spent. In addition, there are factors such as melting points to consider: cadmium melts at 321° C, gadolinium at 1306° C. Of course, materials scientists have ways to assemble the best electrode designs. And eventually it will need to be in a design that can be switched on and off - perhaps with microwaves.

The other test electrode was a palladium or titanium sheet layered with Au or W. Microwave energy could be applied to these electrodes, too. However, in view of window V (production of SDN's) precautions against higher neutron emission rates should be taken in these tests. Larger metals, including platinum impurities in palladium cathodes, have been theorized to give higher rates of SDN formation, and consequently more neutrons emitted. Although electron screening at the (example) Ti / W interfaces might help window I (excess heat production), neutrons from window V are not desired. After initial tests on this electrode design are conducted, fine tuning of the cell may be possible. The cathode could be assembled as a "stack of pennies". The pennies are millimeter - thick wafers of Pd or Ti, each coated with a very thin layer of the heavy metal (e.g. Au or W). These layers might be 1 micron thick, or less.

Use of such cathodes in a $D_2$ gas-discharge cell at either high or low pressures (but low voltage) seems plausible to start with since applied microwave radiation will be tested for optimum intensity and frequency. Pre-loading of $D_2$ into the cathode is another experimental variable to test.

## Conclusions (No Strings Attached)

An attempt has been made to present a comprehensive theory of cold fusion - at least a working hypothesis if not a complete theory, which may not be possible from a single model, nor necessary. As Peter Glück stated, "The keyword...is cooperation: both international cooperation and cooperation of the theories, which have to be considered complimentary and not contradictory"[27].

But though the spectrum of reactions outlined here is not expected to cover all possible cold fusion reactions, it is hypothesized, as stated earlier, that it will account for all the major data in every type of experiment classified under Cold Fusion. That is to say, a cold fusion effect will be seen when a local active site in the lattice harbors long chains of oscillating deuterons (or protons) interacting with metal - atom boundaries. It seems to matter little whether these chains are created by electric loading, gas loading, cavitation loading or ion bombardment. Of primary importance is the local concentration of deuterons, which must be high enough to form the linear chains or trains of mobile deuterons in the first place. This necessary condition was summarized by Jean - Paul Biberian in a recent article [28]. Dr. Biberian, a cold fusion researcher and collaborator with E - Quest Sciences, speaks of "strings of deuterons". In fact, he ties things together well by writing that "This idea [the collective mode oscillation of deuteron strings] has several implications: one is that this is a universal phenomenon."

His conclusion will serve to wrap up this article, too.

*David Moon of Minneapolis, Minnesota received his BS degree from Mankato State College in Mankato, Minnesota. He has taught high school chemistry and physics in Missouri, Texas, Wisconsin, and Minnesota. He is now a security guard at Cray Research.*

## References

**1.** E. Storms, "Chemically - Assisted Nuclear Reactions", *"Cold Fusion"* Magazine (edited by E. Mallove), Vol. 1, No. 3, July/August 1994, pp. 42-53.
**2.** P. Glück (Inst. of Isotopic and Molecular Technology, Cluj-Napoca, Romania), "The SURF-DYN Concept: An Attempt to Solve (or Rename) The Puzzles of Cold Nuclear Fusion", *Fusion Technology*, G. H. Miley, editor, Vol. 24, August 1993, pp. 122-126.
**3.** E. H. Lewis (Chicago, IL), "Plasmoids and Cold Fusion", *Cold Fusion Times*, M. Swartz, editor, Vol. 2, No. 1, p. 4.
**4.** D. Letts, reviewed by C. White, *21st Century Science and Technology*, Carol White, editor, Vol. 7, No. 1, Spr. 1994, p. 75.
**5.** D. Moon, "An Elementary Model of Deuteron Behavior", *Fusion Facts*, H. Fox, editor, Vol. 4, No. 12, June 1993, pp. 11-14.
**6.** E. Storms, "Warming Up To Cold Fusion", *Technology Review*, Massachusetts Institute of Technology, S. J. Marcus, editor, May/June 1994, pp. 19-29.
**7.** R. George, Palo Alto, CA, private communication.
**8.** A. Takahashi, reviewed by J. Rothwell, *Fusion Facts*, Vol. 3, No. 9, March 1992, pp. 11-12.
**9.** A. de Ninno, reviewed by C. White, *21st Century Science and Technology*, Vol. 6, No. 1, Spr. 1993, p. 68.
**10.** J. Yang, et al., "Abnormal Nuclear Phenomena and Possible Nuclear Process", *Fusion Technology*, Vol. 25, No. 2, March 1994, pp. 203-206; Abstract review: *Fusion Facts*, Vol. 5, No. 9, March 1994, p. 11.
**11.** J. O'M. Bockris and R. Sundaresan, Dept. of Chemistry, Texas A & M University, "Electrochemistry, Tritium and Transmutation" (Table 2), Cold Fusion Source Book, (edited by H. Fox), International Symposium on Cold Fusion and Advanced Energy Sources, Minsk, Belarus, May 1994.
**12.** R. George, "Highlights of the Nagoya International Conference of October 1992", *The Cold Fusion Newsletter*, Vol. 1, No. 2, July 1993, pp. 2-5.
**13.** P. K. Iyengar, M. Srinivasan, et al., "Bhabha Atomic Research Centre Studies in Cold Fusion", *Fusion Technology*, Vol. 18, August 1990, pp. 32-94 (ref. p. 34).
**14.** Transient tritium events at Texas A & M University, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, E. F. Mallove, (John Wiley and Sons, 1991), p. 193.
**15.** *21st Century Science & Technology*, Vol. 7, No. 1, Spr. 1994, p. 75.
**16.** W. Greiner and A. Sandulescu, "New Radioactivities", *Scientific American*, March 1990, pp. 58-67.
**17.** G. Chambers, G. Hubler, K. Grabowski, NRL Memorandum Report 6927, 18 Dec 1991, Naval Research Laboratory, Washington, DC.
**18.** J. Kasagi and K. Ishii (Tohoku University), "Emission of Energetic Charged Particles", *Oyou Butsuri (JJAP)* 1993, Vol. 62, No. 7, pp. 710-711 (in Japanese); Abstract review: *Fusion Facts*, Vol. 5, No. 5, November 1993, pp. 14-15.
**19.** *21st Century Science & Technology*, Vol. 7, No. 1, Spr. 1994, pp. 73-74.
**20.** *21st Century Science & Technology*, Vol. 6, No. 1, Spr. 1993, pp. 70-71.
**21.** Y. Kucherov, A. Karabut, I. Savvatimova, "Calorimetric and Nuclear Products Measurements at Glow-Discharge in Deuterium", Scientific Industrial Association, LUCH, Podolsk, Moscow Region, Russian Federation, 1993; reviewed by M. Swartz, *Cold Fusion Times*, Vol. 1, No. 4, p. 10.
**22.** *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, E. F. Mallove, (John Wiley & Sons, 1991), p. 168.
**23.** "Beyond Cold Fusion - A New Window on the Laws of the Universe", C. White - interview with M. Fleischmann and G. Preparata, *21st Century Science & Technology*, Vol. 5, No. 4, Wtr. 1992, pp. 59-65, and p. 69.
**24.** E. Yamaguchi and T. Nishioka, "Direct Evidence for Nuclear Fusion Reactions in Deuterated Palladium", NTT Basic Research Laboratories (Tokyo), *Proceedings of the 3rd International Conference on Cold Fusion*, Nagoya, Japan, October 1992.
**25.** M. I. Martinov, A. I. Meldianov, A. M. Cherepovski, (Kurchatov Atomic Energy Inst.), "Investigation of Anomalous Nuclear Events in Metals Saturated With Deuterium", Cold Nuclear Fusion, Centre of Intersectoral Science, Engineering and Venture, Non-conventional Technologies, Moscow, 1993, pp. 84-91; Abstract review: *Fusion Facts*, Vol. 5, No. 5, November 1993, p. 20.
**26.** D. Moon, "A Microwave for a Macro Phenomenon", *"Cold Fusion" Magazine* (edited by E. Mallove), Vol. 1, No. 2, June 1994, p. 52.
**27.** P. Glück, "Cold Fusion - A Logical Network Approach", *Cold Fusion Source Book*, International Symposium on Cold Fusion and Advanced Energy Sources, Minsk, Belarus, May 1994.
**28.** J - P. Biberian, "An Experimentalist's 'Theory' Of Cold Fusion", *"Cold Fusion"* (WGI Newsletter), No. 8, , pp. 4-5.

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# •BRIEFS•

**CETI Prospects Appear Bright**
Informed sources have told *IE* that Clean Energy Technologies, Inc. of Dallas, Texas is now working collaboratively with two universities, a major utility company, and a U.S. Fortune-500 corporation.

**Hot Fusion Meeting to Feature Cold Fusion!**
The 16th Symposium on Fusion Engineering (SOFE '95) will be held in Champaign, Illinois this year, (October 1-5), attracting some 500—mostly **hot** fusion participants. But unlike past hot fusion meetings, at this one—thanks to the efforts of Professor George Miley, Editor of *Fusion Technology*— there will be a room set aside for poster presentations on cold fusion. There is also to be a discussion panel on cold fusion, featuring Dr. Edmund Storms, October 2 at 8:30 p.m. The session will be called "Low-T Fusion." There may even be some other surprises from the cold fusion camp. (Hint: Someone might bring a demo unit!) Cold fusion researchers who are already scheduled to present posters are Professor Peter L. Hagelstein of MIT and K. Kamada of the National Institute for Fusion Science, Nagoya, Japan.

**SRI Cold Fusion Work Now Funded by Japan's NEDO**
The top-notch cold fusion research program at SRI International in Menlo Park, California, formerly funded by EPRI of Palo Alto, California, is now reportedly funded at a level of $700,000 for the first year by the Japan's NEDO (New Energy Development Organization). The funding shift was caused by cross-the-board cutbacks in many of EPRI's research projects. Dr. Tom Passell is still assigned to cold fusion R&D at EPRI.

**Fullerene Fusion and Electrodynamic Plasma**
*A News Release appearing on newsgroup: sci.physics.fusion, 14 Aug 1995*

A patent on a Warm Fusion Electrodynamic Generator was filed in June of 1995 by American Cold Fusion Engineering and Supply, P.O. Box 191394, Sacramento, CA 95819-1394.

The system utilizes the unique properties of deuterated carbon fullerene molecules as a seed. Orientation in a Magnetic Field produces a vortex and surrounding electrodynamic plasma.

The inventors are developing prototype systems and computer simulations in an effort to reduce to practice both theory and apparatus. All inquiries for additional information are available via post at the above address or at 1-800-713-9345 during business hours.

Warren Cooley, Inventor

**Triode CF Cell?**
An annoucement of a possible new direction in cold fusion electrochemical cells was received 7 August 1995:

**Statement**

Cravens Laboratories in Cloudcroft, New Mexico has recently run preliminary evaluation tests on a three-electrode (triode) Pons-Fleischmann type nuclear fusion cell. Evan Ragland, the inventor of the triode cell, believes the third electrode affords a degree of control of cell loading and fusion rate. Results of initial tests are positively encouraging and provide new insight into cell charactersitics. Confirmation experiments and new exploratory experiments are planned to be conducted. The present work plan is to: (1) Confrim initial experimental results; (2) Evaluate some heretofore unobserved phenomena; (3) Conduct experiments on improved cathode embodiments, and (4) Design and test new electronic control circuits.

From: Evan Ragland Company, 6640 Ahekolo Circle, Diamondhead, MS 39525-3461

**Electric Motor Anomaly Descibed in *Journal of Applied Physics***
A paper just published in the *Journal of Applied Physics*, Vol.77(11), 1 June 1995, pp.6015-6020 by Osamu Ide of (Clean Energy Laboratory[!!], Natural Science Group, Shinegawa, Japan), "Increased voltage phenomenon in a resonant circuit of unconventional magnetic configuration."

It shows, conclusively, that there is an electromagnetic anomaly associated with an electric motor—energy recovery in the capacitor that energizes this motor recovers more energy than it is supposed to! There is no statement about "over-unity" here, but there are *profound* implications of the effects seen and why they totally disagree with the usual mathematics that should decribe this motor. Basically, a difference in the behavior of attractive mode and repulsive mode of the magnets *which should not be!*

This may be a way of getting the larger shocking news out—an understated paper with more to follow. *Journal of Applied Physics* is published by the American Institute of Physics!

One sentence in the paper is a bombshell. Quoting the paper: "These results can be explained from the assumption that the complex movement of the flux could generate a positive EMF: the increase in the recharge voltage is to due to an EMF in the same direction as the discharge current, different from the back EMF caused by Faraday's law."

**Challenge to Relativity?**
Relating, perhaps, to the above, another bombshell paper has just been published in the *American Journal of Physics*, Vol.63, No.8, August 1995, pp.694-705, "Maxwell's equations in a rotating medium: Is there a problem?" by Gerald N. Pellegrini and Arthur R. Swift (the latter of the Dept. of Physics and Astronomy, University of Massachusetts, Amherst, MA).

This paper is a direct challenge to Special Relativity. It proves one of two things about a classic 1913 experiment of Wilson and Wilson that was used to verify the prediction of relativity that "a moving magnetic dipole develops an electric dipole moment." The conclusion of the paper is that Special Relativity does NOT agree with this experiment— and no one has ever challenged the quality of that experiment.

Pellegrini told *IE* that he thinks that all of relativity as well as Maxwell's equations as descriptors of EM radiation are now called into question. [The only reason that that paper was published at all is that the reviewers perhaps thought that the "safe out" was that there *might* be a problem with the quality of the Wilson experiment! —Ed.]

**Cosmology in Crisis**
"Press Digest of *Nature*, issue Dated September 7, 1995" says in its lead summary:

"SPACE: COSMOLOGY IS STILL IN CRISIS (pp.27-31)

The latest news from the orbiting Hubble Space Telescope confirms last year's startling revelations, published in Nature, that the Universe appears to be younger than some of the stars within it (see Freedman en al., *Nature* 371, 757-762; 1994, and Pierce et al., *Nature* 371, 385389; 1994). The report should further allay any doubt that there is something amiss with the currently accepted 'standard model' ofthe Universe.

On p27, N. R Tanvir and D. R T. Robinson from the University of Cambridge, UK, T. Shanks from the University of Durham, UK, and H. C. Ferguson from the Space Telescope Science Institute in Baltimore, Maryland, USA, present new, independent measurements of the distance to the galaxy M96 in the distant Leo I cluster of galaxies., and, through these, they provide estimates of the distance to the far more remote Coma cluster. These distances are used to calibrate the rate at which the Universe is expanding - a value known as the Hubble Constant-which, when extrapolated backwards, gives a date for the 'Big Bang' of around 9.5 billion years ago.

Although earlier than the dates suggested by Freedman and Pierce and their colleagues (who applied similar methods to galaxies in the Virgo cluster), it is still much too late to accommodate the oldest stars. Astrophysicists will now have to re examine assumptions about the Universe relating to the amount of dark matter, and the possibility that the 'cosmological constant'-a fudge-factor introduced by Einstein into his equations, which he later recognized as mistaken- might be real after all.

As with the earlier studies, Tanvir and colleagues measure distance indirectly, by recording the periodic fluctuations in brightness of stars known as Cepheid variables. Because period varies inversely with luminosity, or absolute brightness, the distance to the star can be measured by comparing this value with the brightness as it appears to the observer (in the same way that the apparent brightness of a distant light-bulb of known power can be used to judge how far away it is).

This absolute distance can then be compared with the 'redshift' of the galaxy in which the star resides, a spectral distortion resulting from the

***Continued on page 44.***

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# WASHINGTON WATCH

**Sonoluminescence Funds Cut**

*One of IE's observers noted this C-Span television report:*

The U.S. House of Representatives on July 12, 1995 (at 12:30 p.m.) accepted an amendment by Mike Ward (Democrat of Kentucky) to strip $1 million from the $18.7 billion Energy & Water Appropriations Bill for Fiscal Year '96 that was earmarked for Sonoluminesence Research. Speaking against the amendment was congressman Dana Rohrabacher (Republican of California) who had been responsible for the money being in the bill in the first place.

He cited SL as an example of pure research that could be done with relatively little money yet which showed great promise. Ward did not deny the value of the research, but was opposed to the funds because the Department of Energy hadn't requested them, and because Science magazine had said that the research was unlikely to yield anything worthwhile for fusion applications.

It should be noted that Rohrabacher inserted the funds even though the money would not have gone to his district. The final vote was 276 for, 141 against. Almost all of the House Democrats voted for the amendment, and were joined by about a third of the House Republicans.

**Letter to Senator Dole**

August 28, 1995

Senator Bob Dole
United States Senate
Washington, DC 20510-1601

Dear Senator Dole:

I was delighted to hear your views today at your "Town Meeting" in Concord.

Let me propose for a first order of business that you help the United States to correct a terrible scientific injustice—what we could well call the "Heavy Watergate" scandal of 1989-1995! Since I have heard that you have been following developments in cold fusion energy out of the corner of your eye, please let me suggest several corrective actions to what has happened:

**1.** Encourage the relevant Senate committees to hold immediate hearings on the Department of Energy's *gross mishandling* of the cold fusion discovery, which was announced at the University of Utah on March 23, 1989. On the positive side, these hearings should explore what has been happening since then—scientifically and technologically—in this revolutionary field.

**2.** Investigate how it came to be that vested interests in *hot* fusion and high energy physics, threatened by this discovery, came to sit in judgment on a federally convened panel. This panel then produced in November 1989 the expected negative, rush-to-judgment official report. This highly flawed, biased DOE report was totally indefensible then and remains *the* key obstacle to scientific and commercial progress on cold fusion in the United States.

**3.** Make sure that the current Congressional efforts to severely cut the hot fusion program by as much as 38% *stay on track*. In truth, the tokamak hot fusion program deserves to be eliminated entirely, and I hope it will be with your help. Tokamak hot fusion has no future. It is a widely acknowledged complete waste of taxpayer money, even by many of its original supporters. Over a billion dollars has been wasted on hot fusion since 1989. The reality of the cold fusion discovery and international progress to develop it into the most potent energy source in history means that hot fusion is a dead-end technology. Unfortunately, I have seen a recent report that the DOE is trying to arrange the continuation of funding of the Princeton TFTR reactor experiments, thus further wasting taxpayer money.

**4.** Investigate how it is that dozens of U.S. citizens are currently *being denied their constitutional rights* to have their patent applications in cold fusion technology appropriately reviewed and then patents granted. There is a clear evidence trail of DOE involvement with this outrage. This is intolerable and probably illegal.

**5.** Cold fusion needs no overt Federal funding to go forward. All that needs to be done is to get DOE negative influence out of the way so that the private sector can handle this matter in peace. Of course, scientists at Federal laboratories who wish to pursue research interests in this area should be encouraged to do so, much as their counterparts are now in Japan, Italy, and elsewhere. There are some aspects of cold fusion research that have strong national security implications: in particular, the low-energy production of tritium, which is used in thermonuclear weapons. This is certainly an area in which there should be federal action.

**6.** Finally, I urge you not to treat this matter lightly. It will eventually be a major political issue. It certainly deserves to be.

Thank you for taking time to listen to my concerns, which are also those of thousands of scientists and engineers in the United States.

Sincerely,
Eugene F. Mallove, Sc.D.
Editor-in-Chief, *Infinite Energy*

**Fusion Funds Slashed**

The *Washington Post* of July 26, 1995 reported that under the allocations worked out between the 13 Appropriations subcommittees in the U.S. Senate, funding for solar and renewable energy research was reduced by one-third. There was also a deep cut in the allocation for nuclear fusion research, which left uncertain the future of

***Continued on page 44.***

"Without the super-collider, which Congress killed in 1993, or a collider capable of accomplishing the same mission, Weinberg says, **'Within 10 years, research at the frontiers of science will come to an end.'"**

Physics Nobel laureate Dr. Steven Weinberg, University of Texas

Quoted by National Geographic News Service, June 21, 1995

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# CALENDAR

MIT Cold Fusion Day
January 21, 1995
Cambridge, MA

American Chemical Society
April 3, 1995
Cold Fusion Poster Session
Anaheim, California

ICCF5 – Fifth International Conference on Cold Fusion
April 9-13, 1995
Monte Carlo, Monaco

Low-energy Transmutation Meeting (Private) At Texas A&M University
June 19, 1995

The Second International Conference on New Energy Systems and Conversions
July 31-August 4, 1995
Istanbul, Turkey

The 2nd World Hydrogen Summit
September 28
-October 1, 1995
Montreal, Quebec, Canada

16th Symposium on Fusion Engineering (SOFE '95), *with cold fusion component!*
October 1-5, 1995
Champaign, Illinois

Third Russian Conference on Cold Fusion and Nuclear Transmutation
October 1—8, 1995
Sochi, Russia

Alternative Fuels Conference and Exposition — Sponsored by SAE
December 6-8, 1995
San Diego, California

ICCF6—Sixth International Conference on Cold Fusion
Spring or Summer/1996
Hokkaido, Japan

1996 International World Energy System Conference
June 19-21, 1996
Toronto, Canada

ICCF7—Seventh International Conference on Cold Fusion
1997, autumn-winter
Scheduled for the U.S.


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rate at which the galaxy appears to recede as a function of the expansion of the Universe in general. Redshift provides a relative but not absolute measure of distance. An absolute measure is needed to work out the distance to remote galaxies which, with redshift data on their rate of recession, allows researchers to work out the size of the cosmos and the time elapsed since the Big Bang - always assuming that the underlying assumptions about the structure of space-time are correct, something which is now in question."

***Continued from page 43.***

Princeton's Plasma Physics Lab, which had hoped to build a $1 billion dollar hot fusion reactor (TPX) with federal help.

In March, President Clinton asked for a panel to review the DOE fusion program's viability in view of prospective budget cuts. In July, this panel reported that the US would have to spend $320 million a year to assure even a chance that a practical fusion reactor could be built by the middle of the 21st Century. The U.S. House in August had appropriated $229 million for fiscal 1996; the U.S. Senate wanted to spend only $225 million. A top DOE official, Dr. Davies predicts that such a spending decrease—from the approximately $360 million budget last year— would require the layoff of about 1,300 scientists and engineers working on hot fusion in the U.S.

Dr. Miklos Porkolab, Director of MIT's Plasma Fusion Center, was quoted by *The Christian Science Monitor* newspaper (15 August 1995): "Our partners are very upset about this. My European colleagues tell me that this is the last straw...The danger is that if the U.S. cuts its contribution, European governments may cut theirs..."

*Editor's Comment: In 1989 and in the years thereafter, plasma fusion researchers in the U.S. had every opportunity to take cold fusion research seriously by examining the compendious peer-reviewed literature. They didn't, especially at the MIT Plasma Fusion Lab. Now they are faced with the consequences. —EFM*


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# Hypothesis for cold fusion of hydrogen isotopes within metallic matrices

**by Marshall Dudley**

This hypothesis is put forth in an attempt to explain experiments in which excessive heat and He-4 (in the case of D2) are produced in a metal matrix which has a high saturation of hydrogen or deuterium. Since deuterium is an isotope of hydrogen, hydrogen will be used in this article to denote both H1 and D2, and proton will refer to the nucleus of a hydrogen or deuterium atom. H3 will be referred to as tritium.

When hydrogen diffuses into the metallic matrix, it is not going into the interstices of the crystal. Instead it is combining with the metal ionically. However, since hydrogen only has one electron, when it loses it to the outer shell of the metal, we end up with a H or D nucleus.

Normally an ionic bond creates two atoms attracted to each other electrostatically, but in this case the hydrogen is no longer an atom. Thus instead of two atoms held together electrostatically, the hydrogen nucleus is electrostatically attracted *into* the outer shell of the metal. Once inside the outer shell, the hydrogen nucleus sees part of the charge of the metal's nucleus and is repelled back outside of the outer shell. In the case of palladium, when outside the shell it sees a -1 charge, but when inside the outer shell it sees a +10 charge (the nucleus of the palladium not shielded by the 10 outer shell electrons).

This action I will call "porpoising," since it is similar to how a porpoise swims at the surface of the water. I would expect that the nucleus would be inside of the outer shell only about 10% of the time due to the differences in the charges it sees when inside vs outside the shell.

Since the hydrogen nucleus spends most of it's time outside of the shell, the effect would be that adjacent atoms with a porpoising proton, would tend to have the proton on each to move into an area where it is facing an atom without a proton, or which has the proton facing the other direction. If all the atoms surrounding the palladium have protons, then there would be a diffusion force that would tend to spread out the protons such that each would not have to face another porpoising proton.

Hydrogen nuclei should be able to avoid other head-on nuclei up to a loading of around 80%. The exact loading where this occurs will actually depend on the crystal structure of the metal. Once we exceed this loading there will be sites which have nuclei facing each other on adjacent metal atoms. I would expect this to cause two effects. First I would expect that loading above this critical level to suddenly be more difficult to achieve. Secondly, if this hypothesis is correct I would expect that cold fusion would start occurring only once this level of loading is approached or exceeded.

An interesting observation of this hypothesis is that when the proton is inside the shell, the shell shields its charge from being seen from outside the shell. Thus the proton can suddenly appear outside the shell, without any warning. If two separate but adjacent atoms have porpoising protons, then the protons will both occasionally appear on opposite sides of a shell simultaneously, and at a point in which tunneling would ensure fusion. Thus we avoid the problem of coulomb repulsion through the shielding effects of the outer shell of the metal.

As the nuclei approach each other from opposite sides of the outer shell, coulomb attraction will increase the average density of the electrons in the area between the two nuclei. This increase in density increases the effectiveness of the electrostatic shield between the two hydrogen nuclei.

Once the nuclei are within sufficient range of each other they can fuse through the well known tunneling phenomenon.

In the case of deuterium, the result of this fusion is an He-4 nucleus being created virtually in the middle of the swarm of electrons circling within the outer shell of the metal. Since the original hydrogen nuclei were of a very low energy, and approached each other from opposite directions, this He-4 nucleus will have virtually zero inertia. Now theory normally dictates that the newly created He-4 nucleus will be in a highly excited state. Strong force interactions would normally cause the nucleus to split apart, spitting out a neutron and a gamma ray. But experimental evidence does not support this.

Since one would expect this decay to take place within about $10^{-20}$ seconds, any competing process must occur virtually simultaneously with the original fusion to have any significant effect. Generally speaking, non-strong force mediated modes of energy loss are much slower than the strong force mediated neutron emission.

Lets take a close look at what we might expect as the two hydrogen nuclei approach each other from opposite sides of the outer electron shell. As they near each other, electrons will tend to be drawn into the gap between the nuclei, and once they combine into He-4 nucleus, we could expect a barrage of electrons coming well within tunneling distance of this nucleus.

23.8 Mev is released when the D2 fuses into He-4, and a He-4 nucleus requires 20.6 Mev to split into a tritium and a nucleus. Normally a He-4 nucleus will not capture an electron since that would require additional energy. However in a highly excited state an electron can be captured, and immediately emitted back out as a beta particle. If this beta (plus corresponding antineutrino) is 3.25 Mev or higher, then the original He-4 nucleus will be left with insufficient energy to decay into a neutron and tritium via the strong force. Thus the nucleus will have plenty of time to lose it's energy through phonons, weak electromagnetic interactions, or other weak channels of energy transfer.

Other effects I would expect if this hypothesis is correct are:

- Any disturbance that would disrupt the even distribution of protons would enhance the fusion rate. Thus loading shocks, thermal shocks, mechanical shocks, even magnetic and electrostatic shocks could all enhance the fusion rate until the protons become evenly distributed again.
- Application of electromagnetic waves which would cause random movement of the protons around or between the atoms would enhance the effect. These waves may be in the radio frequency or microwave region, or even in the X-ray region. Even ultrasonic vibration could have an effect. I would expect that there are resonant wavelengths which would have much greater effects than other wavelengths in the same range.
- The start of fusion in a area could generate a shock wave of heat, pro-

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tons, stress, and possibly EM waves which would spread, eventually causing a significant portion of the palladium to "turn on." Some people refer to this as ignition.

- The emitted betas will cause the metal to fluoresce in the soft X-ray region. Although most instrumentation would not detect these X-rays, X-ray film should be easily fogged. Fogging of X-ray film near a CF reaction without any measurable gammas and neutrons would tend to support this hypothesis.

**Over 100% Loading**

Now I will address what would be expected if loading exceeds 100%.

First, due to the repulsion of the opposing protons, we could expect the crystal to possibly take on a new form. It may be a different structure, or may still be the same structure, but with an increase in the atomic spacing due to the coulomb repulsion of the porpoising protons.

Also, not only will virtually all the metal's atoms have a proton, but some will have two of them. Those which have two protons will most likely end up with the protons staying on opposite sides of the atom due to coulomb repulsion. However, if one of the protons is disturbed sufficiently, either by a near miss from the proton of an adjacent atom, or by an external force, such as a resonant EM wave, it could be hurled around the atom so that it ends up on the same side as the other proton. In some cases one could be inside the shell and the other outside the shell, both penetrating the shell at virtually the same spot at the same time. Once again we could expect fusion to occur from tunneling. Thus I would expect that loadings greater than 100% could show a marked rise in the fusion rate, especially if there is some type of disrupting force, such as an EM wave employed.

*Marshall Dudley graduated from the University of Tennessee in 1969, with a BSEE in electronics. He designed nuclear instrumentation for Westinghouse, Nuclear Chicago (later Searle Analytic), and most recently he designed low-background detectors and counting systems for Tennelec, Inc. (now Oxford Instruments) in Oak Ridge, Tennessee. He was a member of the Argus Scientific Team, which went to England in 1992 to study crop circles. His specialty was nuclear detection of any radioactive traces, using low-background equipment he designed, and the use of gamma spectrometer equipment for identification. He is now self-employed in the computer software business, but still has a keen interest in electronics, nuclear physics, and unknown or unusual phenomenon.*

*Commentary*

# How to Miss the Energy Boat

**by Hal Fox**

Soon after the March 23, 1989 cold fusion announcement by Stanley Pons and Martin Fleischmann, strong negative comments emerged that cold fusion could not be real. Much of this negative reaction appeared to come from people in universities who were heavily involved in the continuing development of hot fusion. These comments also came from their influential friends in high places.

Soon the following happened: The ERAB (U.S. Dept. of Energy, Energy Research Advisory Board) cold fusion committee recommended no funding for cold fusion (July, then November 1989); a DOE official, who *had* offered funding for cold fusion, had his budget cancelled and he was given a "lateral promotion," i.e. he was out; cold fusion proposals were declared not acceptable for small business development funding (SBIR grants); and U.S. patent examiners and their supervisor proclaimed that cold fusion did not work. This prevented any patent applications from being approved. In addition, Gary Taubes received a handsom advance from Random House to write a book to destroy cold fusion: *Bad Science: The Short Life and Weird Times of Cold Fusion.*

The end result has been the following: The hot fusion community has had its budget severely diminished this year. Over 100 cold fusion patents have issued in Japan and over 200 have issued world-wide. Cold fusion is being commercialized in the U.S. based— to a large extent—on the patent issued to Dr. James Patterson. Gary Taubes' book is becoming a collector's item because its author is at the top of the list of science writers who were so wrong so fast about cold fusion. The Department of Energy is increasingly talked about as one of several Federal Departments deserving to be dismantled. Some concerned citizens are suggesting that there should be a Congressional investigation of the Office of Patents and Trademarks for its role in the cold fusion affair.

It is interesting to speculate what might have happened if a group of hot fusion scientists from a major university (such as MIT) had suggested to DOE that these famous electrochemists might have an intriguing discovery, and if they had requested that five to ten percent of their DOE-funded work on hot fusion should be devoted to the study of cold fusion. Inspired by their leadership, many other universities *and corporations* would have sought similar funding. Most journals would have published peer-reviewed articles on cold fusion. Officials of the American Physical Society would not have dismissed the first cold fusion international conference (Salt Lake City, March 1990) as being "the death rattle of a dying corpse." Many inventors would have been issued patents by the Office of Patents. The U.S. would have become a world leader in new energy instead of leaving the field nearly exclusively to Japan.

*The most important discovery, which has been a byproduct of cold fusion research, is the monumental finding that radioactive nuclei can be stabilized by low energy nuclear reactions!* Rather than creating new sources of radioactive contamination, by now the nuclear scientists involved in hot fusion could have been well on the way toward solving the problems of radioactive waste management. Now, instead of hundreds of millions of dollars for studying hot fusion, this same group could have been looking at *billions* of dollars to handle processes to ameliorate the radioactivity of nuclear waste. In their attacks on cold fusion and on Pons and Fleischmann, those naysayers in the hot fusion community of scientists not only missed the boat, they have presided over and caused the sinking of their own ship.

The basis of science is not controversy per se, but discovery and the replication and reporting of that discovery. In this age, discovery must also be coupled with appropriate treatment of valuable intellectual property. Therefore, scientists must not only discover and report as scientists, they also must file patent applications for the future benefit of their institutions. There is valuable information in the following paraphrase of Arthur C. Clarke's dictum: If a competent scientist tells you that something *can* be done, he is probably right, if a scientist tells you that something *cannot* be done, he is probably wrong.

The next steps are to build on what we know; to determine with both the-

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ory and experiment what we don't know; to share our new discoveries as soon as is practical; and to continue the development of the new science of cold fusion. One other concept having eternal verity: Forgive those misguided scientists who sought to destroy rather than to build. If not by now, soon they will have "repented." —— "Yessir, I knew it was right all along!"

# NEW PRODUCTS

## JET Technology

JET Technology of Wellesley Hills, Massachusetts has recently been advertising in its periodical, *Cold Fusion Times*, several new products. The company refers to its Series 95 Integrating Multiring™ Calorimeters. These are of two forms: 35 cc capacity, employing a 68020 chip, with an overall sensitivity of 3.5 mW ($14,995) and a 450 cc unit with a 68040 chip, with 0.5 mW sensitivity ($19,995).

The company is also selling a series of π-Notch™ Nickel Reactors. One product is a 0.5-10.5 watt Light Water System ($17,995) and the other, apparently in development, is 500 [watt] to 10.5 kilowatt Light Water System. The company requests inquiries from prospective customers on the latter system and does not advertise a price.

The company refers to π-Notch™ Tested Nickel Electrodes that it can provide, all with a peak drive condition of 2.5 watts input power in the following ranges: (nominal 1.05-1.7 excess) for $1,950; (nominal 1.5 to 2.7 excess) for $2,195; (nominal 2.2-3.1 excess) for $2,495; and (nominal 2.7-6.5 excess)—stated as subject to availability. Prices are FOB Boston, MA with 180-day delivery, (prices subject to change without notice). JET Technology may be reached at P.O. Box 81135, Wellesley Hills, MA 02181.

## NOVA Resources Group, Inc.

NOVA Resources Group, Inc. has shifted its product focus from the objective of larger thermal cold fusion reactors—its EG and ETC product line— to equipment suitable for research and education. In its recent sales literature is describes its IE-Itegrated Electrolytic System.

The IE is a self-contained, entry-level, bench-top product, engineered to provide all of the functions necessary to perform accurate, repeatable and reliable cold fusion experirnents. The IE integrates power electrolysis, calorimetry and stoichiometric capabilities into a rugged, stainless-steel--cased unit, which contains the required power-supplies, pumps, valves, heaters, coolers, sensors and interconnections. The IE occupies approximately the same table space as an open notebook.

Within the interchangeable reaction cartridges, cathodes may be formed by electro-deposition, loaded with hydrogen, and run for excess heat, all at regulated temperatures and currents, with stoichiometry determined to 1% in films as thin as three microns.

### Calorimetry and Stoichiometry

The IE utilizes a quasi-adiabatic, single-phase, variable flow, single or twin calorimetric system, based upon a numericallyintegrating coolant pump-rate servo. The pumped flow varies to maintain a constant temperature rise in the dewar. Stoichiometric results are temperature- and barometric-compensated from a numerically-integrating pump-position servo, which holds a constant pressure in a variable-volume chamber connected with the cell cover gas. A solenoid valve briefly switches the pump output from cooling to stoichiometry.

### Control System

The IE is controlled and monitored by computer. Initially this will be accomplished via a supplied interface to standard laboratory data acquisition hardware and software (LabVEW™). An embedded micro controller system is in debug stages of development and will be available later this year to allow stand-alone operation of individual IE units. Communications between multiple (up to 16) IE units and a central PC is handled by a single, multi-drop, RS232 serial j port. This control arrangement is expected to be ideal for the instructional use of the IE in upper-level undergraduate laboratory courses.

### Coolant Pumps

The IE features extemally-mounted, quick-loading peristaltic tubing pumps. These allow visual detection of coolant leakage and easy replacement of worn tubing. In addition, the operating range of the instrument can be changed simply by switching to a different tubing size. Because only the tubing is wetted by the pumped fluid, the pumps on the IE can be used before and after experiments to transfer metered amounts of electrolytes between the reaction cartridges and storage bottles. The IE pump system is self-calibrating for tubing size and flow characteristics. Software routines compensate for the pulsation inherent in peristaltic pumping.

### Operation

Experiments carried out in the IE use the electrochemical cartridges (described on p.4) for all reacting materials. First, after a solvent rinse, the plating anode is threaded onto its conductor rod inside the cartridge and the cartridge, with its sealing ring, is inselted into the IE. Then, a length of acid-resistant tubing is loaded into the pump-head and plating solution is pumped into the cartridge. The recombiner and cartridge cap are installed, and the hydrogen line from the stoichiometric subsystem connected to the cap. Next, after re-looping the coolant tubing through the pump and running a tube-size calibration software routine, deuterium gas, at atmospheric pressure, is drawn into the bladder chamber from an external syringe, through the septum. After a brief wait while the oxygen in the air trapped in the cartridge is catalytically combined with deuterium, electrolysis power is applied under software control until the desired cathode thickness is plated. The previous steps are then reversed, and repeated, substituting the reaction anode and reaction electrolyte for the plating materials. Again, after oxygen removal, power is applied to the cell, this time at low current density for cathode loading, with the coolant temperature reduced via the thermoelectric module in the coolant reservoir. Once the stoichiometer shows that the programmed loading ratio has been achieved, the coolant temperature is increased to the desired operating point, high-power or pulsed current is supplied to the cell and excess energy production sought through calorimetry.

Safety features include: •cannot be pressurized-no gas build-up; •interlocked lid and safety shield; * low voltages (outside power supply cage); and *heavy stainless steel enclosure. Prices ($US) are stated (at the time of ICCF5) as $4,500 for the IE Twin, a dual calorimeter/stochiometer subsystems; $3,000 for the IE Single, a single calorimeter/stochiometer subsystem.

NRG may be reached at 1553 Platte Street, Suite 301, Denver, CO 80202.

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# Cold Fusion: A Study Involving the Fusion of Ions

by Michael P. Belcher

*The following report of his science project research was prepared by high school student Michael P. Belcher of Princeton, West Virginia. Conducted in 1993-1994, this project won first place at the county level and allowed Michael to compete at the West Virginia State Science Fair as well as at the 45th International Science and Engineering Fair in Birmingham, Alabama. At both the state and international levels, this project was reviewed with some skepticism, as might be expected, but Michael was encouraged by some observers to continue his work in this promising field. The certainty or uncertainty of the conclusions of this work is not as important as the initiative and demonstrated boldness of a young scientist working on the frontiers of knowledge. We hope this will inspire other young investigators to get to work in this field. —**The Editors***


**Abstract**

The purpose of this project was to determine if a proton will fuse with the nucleus of a positively charged ion in a cold fusion type electrolysis reaction.

Electrolytic cells were set up using platinum wire for the anode and nickel for the cathode. Cells using these electrodes reportedly produced cold fusion type reactions. The cells used electrolytes of de-ionized water with potassium carbonate and sodium nitrate. I hypothesized that the hydrogen ions generated at the cathode would fuse with the positively charged ions of the electrolyte inside the crystal lattice structure of the metal. If this process occurred, an element with one more proton than the positive ion of the electrolyte would be given off into the solution. The electrolyte was tested regularly for the presence of calcium and magnesium. Because the electrolytes were composed of de-ionized water, the presence of either of these elements would indicate that ionic fusion occurred.

Neither element was detected in this series of experiments, leading to the conclusion that ionic fusion did not occur under the conditions present in these experiments. Further research is being conducted to attempt to determine if ionic fusion will occur under other conditions.


## EXPERIMENTER'S CORNER

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### I. Introduction

Cold fusion: the name itself seems to be a contradiction in terms. For years, scientists all over the world have tried to attain the fusion of two hydrogen nuclei. This process should yield an incredible amount of energy, according to Einstein's famous equation $E=MC^2$ which describes how a nuclear reaction releases energy through the conversion of mass into energy. In nuclear fusion, when any two particles combine, some of the combined mass of the particles is converted into energy. However, to make the slippery hydrogen atom fuse and gain energy back out of the system, researchers have tried everything from magnetic confinement of plasma to bombardment with lasers. That is, until March 23, 1989, when two scientists stunned the world by claiming to have produced nuclear fusion in a test tube at room temperature.

The scientists were B. Stanley Pons and Martin Fleischmann, working at the University of Utah They claimed to have seen excess heat production in a simple electrolytic cell containing deuterium, or a heavy isotope of hydrogen. Pons and Fleischmann reasoned that the only process that could produce the kind of excess heat they were seeing was nuclear fusion. About the same time this extraordinary claim was made, another scientist at Brigham Young University, named Steven Jones, had claimed to have detected radiation from his Pons and Fleischmann type cells.

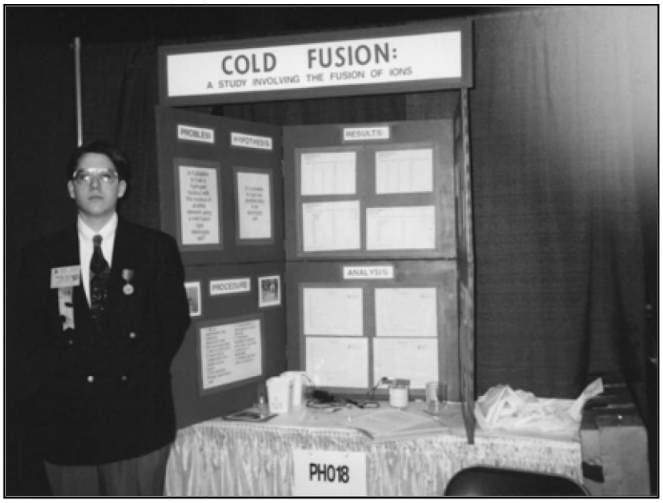
Both groups had worked independently of each other, but both had the same claim: nuclear fusion in a bottle. However, while Pons and Fleischmann detected heat in their cells, Jones and his group noticed only neutron radiation. Later, groups of scientists claimed to have observed the same kinds of results in cells using ordinary, or light water instead of deuterated water. Ordinary hydrogen nuclei cannot fuse, at least not in the usual sense, for reasons that will be described later.

This project endeavored to research some of the claims behind cold fusion. It was based on some of the factors reported to cause light water fusion. By testing these claims, this project helped further understanding about what goes on in light water fusion.

### II. Research

The hypothesis cold fusion researchers such as Pons and Fleischmann developed was an interesting and quite simple one. The principle behind cold fusion is rooted in the operation of electrolytic cells.

Electrolysis is surprisingly elegant in design. Electricity passes through water, flowing from the cathode, or negative electrodes to the anode, or positive electrode. In short, the electrons encourage the hydrogen hydroxide molecules to split into hydrogen ions and hydroxide ions. The hydroxide ion picks up an electron from the hydrogen it was bonded with and becomes negatively charged. It is attracted to the anode where the oxygen picks up another electron, becomes stable, and gives up the hydrogen ion. It then bubbles off as a gas. The hydrogen ions, stripped of their electrons, become positively charged nuclei. These nuclei travel to




Michael Belcher of Princeton, West Virginia at award-winning science fair project on cold fusion.

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the cathode where they recover an electron each. They then form hydrogen gas which is given off. However, cold fusion relies on the chance that the hydrogen will not be given off but will enter the cathode and fuse with another nucleus.

Cold fusion researchers used palladium for their cathodes. Palladium has long been known to attract hydrogen and also has a tightly packed face-centered cubic crystal lattice structure. The scientists hypothesized that the hydrogen ions would become trapped within the cubic structure of the palladium and be squeezed together. They hoped that the ions would become so close that they would overcome the Coulomb barrier and fuse through a process called quantum tunneling.

The Coulomb barrier can be thought of as the repulsive barrier between two particles of like charges. For example, two magnets' like poles cannot come together by themselves, because like charges repel. However, the magnets can come together if someone forces them by pushing them together.

This is where quantum tunneling comes in. If two particles with the same charge (for example, magnets) are held in place for a long enough time period, there is a small chance that they will overcome the repulsive barrier and come together. The same applies to the theory behind cold fusion. If two hydrogen nuclei are held in close proximity long enough, there is a small chance that the nuclei will tunnel through the Coulomb barrier and fuse.

When Pons and Fleischmann set up their experiments, the variable they intended to measure was excess heat. Fusion in the cell would cause the water in the cell to heat up. The scientists measured the heat using normal thermistors placed inside the cell and outside the cell in a constant temperature water bath. When they compared the temperature inside the cell with the temperature outside the cell, they found that the cell had heated up during the course of their experiment.

Recently, other researchers have claimed to have reproduced Pons and Fleischmann's original experiments with one dramatic change: the electrolyte was composed of ordinary water, which lacks deuterium. Ordinary hydrogen nuclei, or protons, cannot fuse to form helium because the nucleus of a helium atom cannot hold itself together without a neutron.

Two separate groups are given credit for producing light water fusion. Both groups have claimed to have seen excess heat in their experiments. The first group is Bush and Eagleton of California Polytechnic, who, in conjunction with Reiko Notoya in Japan, reported the results on which this project is based. The second person is chemist Randall L. Mills.

Mills developed a way to explain the excess heat in his experiments. Mills and his associates stated that the electron shells around the hydrogen and the ions of the electrolyte collapsed or shrunk, releasing energy. However, this theory has not been proven by any means except through the detection of excess heat.

Bush, Eagleton and Notoya have created another theory explaining how the excess heat is generated. Their cells contain light water spiked with potassium carbonate for the electrolyte and a form of porous nickel for the cathode. They suggest that the ionized hydrogen is fusing with the ionized potassium to produce calcium. They reported that they have detected excess heat and calcium in the electrolytes of some of their cells.

Michael Belcher at work at home at his cold fusion lab bench.

**III. Discussion**

Conventional nuclear fusion (the fusion of two isotopes of hydrogen) has three major byproducts; energy in the form of heat, radiation, and helium. (Some reactions result in the production of tritium which is the isotope of hydrogen with two neutrons.) Different reactions yield different types of byproducts, however, all fusion reactions include at least one of these three. The different types of fusion reactions are illustrated in appendix A

Most cold fusion researchers have only measured two of the three byproducts produced in fusion. Heat and radiation are probably the easiest to detect. Only a few researchers have made the effort to look for helium The Bush and Notoya experiments used excess heat measurements to substantiate their claims of cold fusion. The detection of calcium in the electrolytes merely helped explain what could be going on in the cells.

This project goes a step further than the Bush and Notoya experiments to explain the supposed occurrence of light water fusion. In electrolysis, both the water molecules and the molecules of the electrolyte are broken up. The positive ions of the electrolyte are attracted to the cathode along with the hydrogen ions. If the two ions became trapped in the cathode, they should fuse just as deuterium nuclei would fuse with each other. The forces inside the electrode should be strong enough to allow the hydrogen ion or proton to burrow through the electrolyte's remaining electron shell and fuse. If this process occurred, an element with one more proton than the positive ion of the electrolyte should be formed. This process would not be conventional cold fusion, which involves the fusing of two isotopes of hydrogen. This process would be cold fusion in the sense that it is a type of nuclear fusion occurring at relatively low temperatures, however, it would be more appropriately dubbed ionic fusion. If ionic fusion is taking place there should be an increase in the concentration of the elements with more protons. This is the basis of my experimentation.

**IV. Experiment**

I ran six experiments testing three different electrolytes. I used de-ionized water for the solvents so that none of the element I was testing for would be already present in the cell. Each experiment consisted of an experimental cell through which electricity was run and a control cell made if an identical material to insure that no leeching occurred from the cells or any other sources.

Before I ran voltage through the experimental cell I took a control sample of the electrolyte and another right after I poured the electrolyte into the cell. This allowed me to have a double control of the cell and the electrolyte itself.

Every day while the experiment was running, I took a 10mL sample from the experimental electrolyte. I had these samples analyzed, with the spectrophotometer on hand at the

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local hospital, for the element with one more proton than the positive ion of the electrolyte. If any of the test elements had been detected, it would have been proof that ionic fusion was occurring in the cell. Even if the experimental electrolyte had become contaminated somehow, the contamination would have shown up in the control samples.

**Experiment Configurations:**

Experiment 1

Electrolyte: 0.1 molar concentration of $MnCl_2$ x $4H_20$ in de-ionized water, 500 mL per cell
Time cell in operation: 5 days
Input power: 6V DC 100 mA
Anode: Platinum wire, approx. 10 cm long
Cathode: Fused nickel powder, approx. 3 cm in diameter, 2-3 mm thick
Sample Amount: 10mL
Sample Interval: 1 day
Test Element: Iron (Mn+lp)
Results: See project notebook, experiment 1
*Comments:*
Results of experiment 1 discarded due to possible contamination from alligator clip holding nickel. On the third day of the experiment, a large increase of iron was recorded. About that time, the alligator clip holding the nickel had dissolved and dropped into the solution.

Experiment 2

Electrolyte: 0.1 molar concentration of $NaNO_3$ in de-ionized water, 500 mL per cell.
Time cell in operation: 12 days
Input power: 6V DC 150 mA, changed to 3V DC 150 mA
Anode: Platinum wire, approx. 10 cm long
Cathode: Nickel foil, approx 2.5 cm by 1.3 cm; changed to fused nickel powder approx 3 cm in diameter, 2-3 mm thick later in experiment
Sample amount: 10 mL
Sample interval: 1 day, .5 day later in experiment
Test element: Magnesium (Na+lp)
Results: No significant amount of the test element was detected in any sample.
*Comments:*
At various times during experiments 2 and 3, variables were changed in an attempt to isolate proper conditions for ionic fusion to occur. Samples were tested about every five days. I made sure that the test results were negative before I changed a variable. On day six of the experiments, I changed the cathodes in both experimental cells to fused nickel powder. I noticed that the hydrogen bubbles being given off at the cathode seemed to be sliding off the surface of the nickel foil I had been using. I changed the cathodes to fused nickel powder because they had a rougher surface that appeared to trap the hydrogen. Later, I reduced the input voltage because I thought the input power may be too great to allow the ions to fuse. However, the results of both experiments remained negative.

Experiment 3

Electrolyte: 0.1 molar concentration of $K_2CO_3$ in de-ionized water, 500 mL per cell.
Time cell in operation: 12 days
Input power: 6V DC 150 mA, changed to 3V DC 150 mA
Anode: Platinum wire, approx. 10 cm long
Cathode: Nickel foil, approx 2.5 cm by 1.3 cm; changed to fused nickel powder approx 3 cm in diameter, 2-3 mm thick later in experiment
Sample amount: 10 mL
Sample interval: 1 day, .S day later in experiment
Test element: Calcium (K+lp) Results: No significant amount of the test ele ment was detected in any sample.
*Comments:*
See experiment 2 comments.

Experiment 4

Electrolyte: 0.6 molar concentration of $NaNO_3$ in de-ionized water, 200 mL per cell
Time cell in operation: 13 days
Input power: 6V DC 150 mA
Anode: Platinum wire, approx. 10 cm long
Cathode: 1 mm diameter nickel wire looped around anode
Sample Amount: 2 mL
Sample Interval: 1 day
Test Element: Mg (Na+lp)
Results: See project notebook, experiment 4
*Comments:*
This experiment appeared to have worked. A large increase in the amount of magnesium in the electrolyte was detected. However, some problems arose from this particular experiment. The electrode thinned, dissolved, and broke off on several different occasions The nickel could have dissolved for several different reasons. The most likely cause is that in the electrolysis of the sodium nitrate, nitric acid ($HNO_3$) was formed. This acid probably dissolved the nickel.

However, another possibility is that the cell could have succeeded in producing magnesium. The magnesium could have taken the place of the nickel as the cathode and caused the nickel to dissolve in the solution. Evidence that supports this explanation is the fact that the nickel dissolved no further once it separated from the source of the current and dropped into the solution. Nitric acid should have continued to break the nickel down even after it dropped to the bottom of the cell.

The dispersion of the nickel into the electrolyte would normally not create a problem, however, the nickel that was used was not completely pure. This led to the possibility that the nickel wire contained magnesium and that the dissolved cathode was the actual source of the magnesium. In an attempt to find out whether the cathode was the source of the magnesium, different pieces of nickel wire were dissolved in nitric acid and the solutions were tested for magnesium.

I had not kept a piece of nickel from the original strand of wire that I used to make the electrodes. All of the nickel wire had been used as a cathode in a cell. Therefore, in order to obtain a control piece of nickel, I dissolved a piece of the cathode that had previously been used in experiment 5. I then took a piece of nickel from the $NaNO_3$ cell (cell A) and dissolved it. The piece of control nickel actually had a higher amount of magnesium in it than the piece of nickel being tested. I disregarded these results due to possible contamination of the control cathode.

Next, I dissolved a piece of nickel from the top of the experimental cell from experiment 6. This piece of nickel had not been immersed in the cell and should not have been exposed to any magnesium. I used this piece of nickel as a control. I then took a piece of unimmersed nickel from the top of experiment 4's cathode and a piece of the wire that had been immersed from the bottom.

The nickel from both the bottom and the top of the $NaNO_3$ cell had a higher amount of magnesium than the control from the other cell. The control also contained a significant amount of magnesium. However, the piece of nickel from the top of the cathode in cell A contained a lower amount of magnesium than the bottom piece. If all of the magnesium came from the nickel, the amount of magnesium should have been uniform throughout the wire. However, the magnesium levels were somewhat higher in the piece of nickel that had been immersed in the electrolyte than the levels found in the unimmersed piece.

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The occurrence of ionic fusion in the cell would have taken place in the electrode and produced magnesium. Some of the magnesium would then leave the nickel and diffuse throughout the solution. The fact that the magnesium levels were higher in the piece of immersed nickel than those of the other piece from the same cell suggests that magnesium might have been formed in part of the nickel as a result of ionic fusion. However, the nickel definitely contained magnesium, and so these conclusions cannot be entirely valid due to possible contamination from the dissolved nickel. But, ionic fusion is a possible cause of the magnesium increases.

Experiment 5

Electrolyte: 0.6 molar concentration of $MnCl_2 \times 4H_2O$ in de-ionized water, 200 mL per cell
Time cell in operation: 3 days
Input power: 6V DC 100 mA
Anode: Platinum foil
Cathode: 1 mm diameter nickel wire looped around anode
Sample Amount: 2 mL
Sample Interval: 1 day
Test Element: Fe (Mn+lp)
Results: See project notebook, experiment 5

*Comments:*

The nickel cathode again dissolved and broke off into the cell, as in experiment 1. The results of this experiment were negative in that no increase of iron was detected in the electrolyte.

Experiment 6

Electrolyte: 0.6 molar concentration of $K_2C0_3$ in de-ionized water, 200 mL per cell
Time cell in operation: 9 days
Input power: 6V DC 150 mA
Anode: Platinum wire, approx. 10 cm long
Cathode: 1 mm diameter nickel wire looped around anode
Sample Amount: 2 mL
Sample Interval: 1 day
Test Element: Ca (K+lp)
Results: See project notebook experiment 6

*Comments:*

This experiment appeared to have worked. A small increase in the amount of calcium in the electrolyte was detected. However, the increase was a very small one. The spectrophotometer that was used to make measurements was not very accurate in the low ranges in which these increases were recorded.

The increases of calcium should not have come from the nickel cathode, because it did not dissolve into the electrolyte. The electrode was also tested toward the end of the experiment in the same manner that the cathode was tested in experiment 4. The nickel from both cells contained a very minute amount of calcium. The amount that was detected could not have accounted for the increase of calcium levels in the electrolyte.

Contamination from outside sources was also considered. A preliminary electrolytic sample was taken in addition to another sample from a control cell at the end of the experiment, as was in all other experiments. Electrolyte additions were made from time to time to the experimental and control cells from the original electrolyte solution so as to keep the levels in the cell the same. Therefore, another sample from the potassium carbonate electrolyte was taken toward the end of the experiment to account for any outside contamination or leaching of calcium from the container. All calcium results from the control samples were negative.

The fact that the calcium measurements might not have been entirely accurate hinders the drawing of an entirely valid conclusion. However, this experiment indicated that ionic fusion might have occurred in the cell.

## V. Conclusions

Most of the experiments yielded negative results in that the test element was not detected in the electrolytes. The most likely causes for this are that the conditions were not appropriate for ionic fusion to occur in the cell or ionic fusion is not possible.

Some experiments appeared to have yielded positive results. There were possible errors in these experiments that could explain the apparent success of the tests. All efforts were made to eliminate the possibility of faulty results due to mistakes in the procedure, however, no experiment is foolproof. The positive results could have come from several different factors, such as leaching from the electrode or lack of sufficient sensitivity in the spectrophotometric measurements.

Because most factors of error were ruled out, it can be concluded that ionic fusion is a possibility in some instances. Good science dictates that all possibility of error must be ruled out before a definite conclusion can be drawn. Therefore, I can only conclude that ionic fusion might have occurred in some cells. I have learned that I must control more factors in my experimentation before I can determine if ionic fusion is possible.

## VI. Further Research

More extensive research must be conducted in order to determine if ionic fusion is possible. Extended research should include a more sensitive measurement system to decrease the chances of inaccurate readings. A different type of electrolyte, one that would not corrode or dissolve the electrodes should also be considered. Highly pure electrodes would help to insure accurate measurements even if the electrodes did dissolve into the electrolyte. Finally, research into what electrode configurations yield the best results should be performed.

## VII. Sources and Acknowledgments


—Bishop, Jerry E., "Cold Fusion: Fact or Fantasy." *Popular Science*. August 1993. pp. 47-51, 82.
—Close, Frank. *Too Hot to Handle*. Princeton University Press: Princeton, NJ. (1991).
—Glasstone, Samuel and Lovberg, Ralph H. *Controlled Thermonuclear Reactions*. D. Van Nostrand Company, Inc., Princeton University Press, Princeton, NJ. (1960).
—Mills, Randall L. & Kneizys, Steven P. "Excess Heat Production by the Electrolysis of an Aqueous Potassium Carbonate Electrolyte and the Implications for Cold Fusion," *Fusion Technology*, Aug. 1991. pp. 65-81.
—Peat, F. David, *Cold Fusion*, Contemporary Books: Chicago IL. (1990).
—Personal Correspondence with Bob Schick, associate to Randall Mills (on March 20, 1994).
—Princeton [West Virginia] Community Hospital: use of Dimension Spectrophotometer during course of project to analyze samples.


## Sonoluminescence in the basement lab: Not so easy, but try it!

**On the vortex-l@eskimo.com forum: by Scott Little, EarthTech International**

We have experimented with single bubble sonoluminescence in our lab and have found it rather difficult to observe. The work has been done mostly by Dave Clifton, who is somewhat more patient than I am. We followed carefully the procedure outlined in the recent "Amateur Scientist" article in *Scientific American* and even purchased the necessary piezo transducers from the supplier listed therein.

We did succeed in observing the effect on two or three occasions out of something like 50 attempts. Each attempt involved boiling some distilled water to degas it, sealing the container after boiling, cooling the freshly boiled sealed-up water to room temperature, then opening the container, carefully decanting the degassed water into the spherical flask equipped with piezo transducers, tuning the L-C oscillator to resonate at the audio resonance of the flask, squirting in a small shot of water to create seed bubbles, playing with the signal amplitude, all the while squinting at either the scope display of a piezo microphone signal (the onset of SBSL is characterized by a low amplitude higher-frequency signal that appears superimposed upon

***Continued next page.***

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## Tabular Experimental Results from Michael Belcher's Lab Notebook

Experiment 2
Sample Results (mg/dL)

| Sample # | Time (Days) | Experimental Mg | Control Mg (mg/dL) |
|---|---|---|---|
| 1 | 1 | -0.1 | -0.1 |
| 2 | 2 | -0.2 | |
| 3 | 3 | -0.1 | |
| 4 | 4 | -0.1 | |
| 5 | 5 | -0.1 | |
| 6 | 6 | -0.1 | |
| 7 | 7 | -0.1 | |
| 8 | 8 | -0.1 | |
| 9 | 9 | -0.1 | |
| 10 | 10 | -0.1 | |
| 11 | 11 | -0.1 | |
| 12 | 11.5 | -0.1 | |
| 13 | 12 | -0.1 | |
| 14 | 12.5 | -0.1 | -0.1 |

Experiment 3
Sample Results (mg/dL)

| Sample # | Time (Days) | Experimental Ca | Control Ca |
|---|---|---|---|
| 1 | 1 | 1.6 | 1.6 |
| 2 | 2 | 1.5 | |
| 3 | 3 | 1.5 | |
| 4 | 4 | 1.5 | |
| 5 | 5 | 1.4 | |
| 6 | 6 | 1.4 | |
| 7 | 7 | 1.4 | |
| 8 | 8 | 1.4 | |
| 9 | 9 | 1.4 | |
| 10 | 10 | 1.4 | |
| 11 | 11 | 1.4 | |
| 12 | 11.5 | 1.3 | |
| 13 | 12 | 1.4 | |
| 14 | 12.5 | 1.3 | 1.3 |

Experiment 6
Sample Results (mg/dL)

| Sample # | Time (D/HH:MM) | Experimental Ca | Control Ca | Ca in Cathode |
|---|---|---|---|---|
| 0 | K2CO3 Control | 1.8 | 1.8 | 0.6 |
| 1 | 1/00:00 | 1.8 | | 1 |
| 2 | 1/00:30* | 1.8 | | 0.6 |
| 3 | 2/ | 1.8 | | 0.8 |
| 4 | 3/ | 1.9 | | |
| 5 | 4/ | 1.9 | | |
| 6 | 5/ | 2 | | |
| 7 | 6/ | 2.1 | | |
| 8 | 7/ | 1.4 | | |
| 9 | 8/ | 1.5 | 1.2 | |
| 10 | 9/ | 1.5 | 1.3 | |

* After inverse voltage input

Experiment 4
Sample Results (mg/dL)

| Sample # | Time (D/HH:MM) | Experimental Mg | Control Mg | Mg in Cathode |
|---|---|---|---|---|
| 0 | NaNO3 Control | -0.1 | -0.1 | 1.1 |
| 1 | 1/00:00 | -0.1 | | 15.1 |
| 2 | 1/00:30* | -0.1 | | 22.7 |
| 3 | 2/ | -0.1 | | 14.1 |
| 4 | 3/ | 0 | | |
| 5 | 4/ | -0.1 | | |
| 6 | 5/ | 0 | | |
| 7 | 6/ | -0.1 | | |
| 8 | 7/ | 0.3 | | |
| 9 | 8/ | N/A | | |
| 10 | 9/ | 0.2 | | |
| 11 | 10/ | 0.3 | | |
| 12 | 11/ | 0.5 | | |
| 13 | 12/ | 1.2 | | |
| 14 | 13/ | 1.3 | | |
| 15 | 14/ | 1.3 | | |
| 16 | 15/ | 1.1 | -0.1 | |
| 17 | 16/ | 0.8 | | |
| 18 | 17/ | 1.2 | -0.1 | |

Experiment 5
Sample Results (mg/dL)

| Date | Time (D/HH:MM) | Experimental Fe | Control Fe |
|---|---|---|---|
| 4/20 | MnCl2 Control | 28 | 28 |
| 4/20 | 1/00:00 | 16 | |
| 4/21 | 1/00:30* | 10 | |
| 4/22 | 2/24:00 | -27 | 13 |

4/22-Nickel Dissolved
Experiment Discontinued

Cathode Test Results (mg/dL)

| Nickel Type | Test | Mg Results | Ca Results |
|---|---|---|---|
| Control: Top of Cathode NaNO3 | Mg,Ca | 22.7 | 1 |
| Test: Bottom of Cathode NaNO3 | Mg | 27 | |
| Control: Top of Cathode K2CO3 | Mg,Ca | 15.1 | 0.6 |
| Test: Bottom of Cathode K2CO3 | Ca | | 0.8 |

***Continued from page 51.***

the primary drive signal) or the flask itself in a darkened room. On the few occasions that these many parameters were simultaneously within range, we were rewarded with a barely visible blue glowing pinpoint in the center of the spherical flask that typically persisted just until we'd turn away to get somebody else's attention.

Needless to say, we have temporarily despaired of our hopes to create a stable, efficient SL device to perform energy balance experiments upon. If anyone can provide any assistance with our problems it would be greatly appreciated. We have already made a half-hearted attempt to contact Putterman et al via email and apparently do not have the correct addresses.

If this experiment is any example, it is distinctly possible that the Potapov effect is rather difficult to observe as well.

***Readers who would like to experiment with sonoluminescence should look at the "Amateur Scientist" section of Scientiffic American, February 1995, pp.96-98, "Producing Light from a Bubble of Air," by Robert A. Hiller and Bradley P. Barber of Seth J. Putterman's laboratory at the University of California at Los Angeles.– EFM***

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**BOOK REVIEW — by Jed Rothwell**

## *A Dialogue on Chemically Induced Nuclear Effects—*

*A Guide for the Perplexed About Cold Fusion*,

by Nate Hoffman. Published by the American Nuclear Society (ANS) with support from the Electric Power Research Institute (EPRI), 1995, ISBN:0-89448-558-X, 223 pages (69 pages as appendices), $25.00.

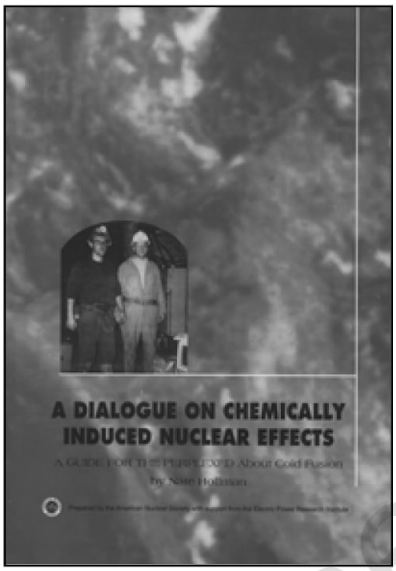
***WARNING: OPEN THIS BOOK CAREFULLY!***

***When you take this book of the mailing box, be careful. There is a one-page addendum you might drop on the floor or leave in the box. Don't lose it, because it contains the most important statement in the book:***

"**ADDENDUM**
"Comments were made in this text that the work performed by SRI INTERNATIONAL was difficult to examine in detail because that lab was reticent to share experimental details of a potentially profitable field of research. This experimental secrecy was partially lifted by the following Report to EPRI:
"McKubre, M. C. H., et al., 'Development of Advanced Concepts for Nuclear Processes in Deuterated Metals,' TR-104195, Research Project 3170-01, Final Report, August 1994."

This is a strange little book. It well written in some parts, with knowledgeable, in-depth, analysis. Yet elsewhere the author makes factual errors that might easily have been avoided. Some of his mistakes are mind-boggling, like his suggestions that chemical supply companies sell used moderator heavy water from CANDU fission reactors, or that no researcher in any cold experiment has ever measured true rms power. The focus of the book is wrong. It covers a few backwater aspects of cold fusion. It describes a handful of unimportant, botched experiments while it ignores the real work. The most important fact about cold fusion is that it produces excess heat beyond the limits of chemistry. As Fleischmann says, "heat is the principal signature of the reaction." In most experiments, heat is the easiest parameter to measure, giving the highest signal-to-noise ratio. Yet Hoffman does not discuss any experiments in which excess heat was observed. He censors them out, he pretends they do not exist. This eliminates most of the literature. In the second paragraph of the book, Hoffman says that Pons and Fleischmann claimed excess heat, but that is the last we hear about the subject until the closing remarks. He never says that hundreds of other scientists replicated their findings. He never mentions any particulars about heat. There is no discussion of power; net energy; energy versus mass (megajoules per mole of cathode material); power density; temperature; current density and other triggering mechanisms; or metallurgical conditions and surface treatments required to generate excess heat.

This censorship of anything relating to heat is carried to absurd extremes. A short chapter on calorimetry, titled "Possible




Artifacts Associated with Heat Measurements in Palladium/Deuterium Systems" contains only speculation about hypothetical errors, and no actual calorimetric data from any experiment. Chapters that deal with things like neutrons, charged particles, and helium contain references to the literature, samples of data from published experiments, and comments from researchers. But the chapter on calorimetry has no data from any cold fusion paper, even though the majority of papers in the field deal with this subject. Other chapters have extensive bibliographies; this chapter lists four items from outside the literature (including a reference to the Fluke instrument company Catalog), and this whimsical little note:

"Note: Listed bibliography concentrates solely on artifact considerations, not on the numerous studies of heat generation in deuterium / solid systems."

McKubre observed megajoules of excess heat per mole at high signal to noise ratios, using superb calorimeters, so Hoffman censored him. This *Addendum* lists only McKubre; it should have listed papers from Pons and Fleischmann, Kunimatsu, Storms, Oriani, Gozzi, Aoki, Huggins, Okamoto and dozens of other top scientists who Hoffman ignored. The *Addendum* of missing papers and data should have been as long as the *Fusion Facts* bibliography (more than 100 pages). On the back page, reviewer Tillbrook of the ANS describes this book as "wide in scope." It is just the opposite. This is a classic case of academic tunnel vision, in which the author refuses to examine 99% of the published papers because he disagrees with their conclusions.

Let us start at the end, and look at this afterthought Addendum carefully, because it epitomizes the whole book.

1. Hoffman asserts that SRI's work has been secret until the Final Report was published in August 1994. This is incorrect. SRI revealed copious details in 1991 at the Second International Conference (ICCF2) (1) at ICCF3 in 1992 (2) at ICCF4 in 1993(3); in the *Journal of Electroanalytical Chemistry* article of 1994 (4); and at ICCF5. McKubre and others have discussed this work in formal lectures at MIT(5) (M.I.T. EECS-LECTURE, Sept. 24, 1992), Texas A&M, and elsewhere. The Final Report does contain new details, but it does not have any surprises or new information that would impact upon the arguments in this book. Everything Hoffman needed to know was published five years ago. On page 75, he gives one example of one detail that SRI has not shared. Just because SRI did not share one detail he wanted, that does not mean he can justify ignoring the great mass of information they <u>did</u> share.

2. Hoffman asserts that SRI's work was difficult to examine when the book was written. Yet every other review of the field written in the last five years discusses SRI's work, and dozens of other papers reference it.

3. He says that the lab was "reticent to share experimental details" because this field is potentially profitable. He asserts as a general rule that you cannot get sufficient scientific information in areas of research driven by industrial profits. Yet, most research and development in most fields is for profit. Countless journals, magazines and textbooks are devoted to purely commercial R&D areas like semiconductors and oil refining.

4. This book was published with support from EPRI. The Forward was written by Thomas Schneider of EPRI. EPRI paid for McKubre's research at SRI and Wolf's research at Texas A&M, and it published the Final Report that barely made it into the *Addendum*. Schneider has been a known behind-the-scenes bitter opponent of cold fusion for many years, but he must have been aware of McKubre's numerous publications over the years, and he must have realized that McKubre's data contradicts Hoffman's conclusions. He should have told Hoffman to include <u>something</u> about McKubre. Schneider and Hoffman want to present a one-sided picture. They want to prove that all positive results are due to "an unidentified error or artifact," as Schneider puts it.

McKubre's experiments are among the most important in this field. McKubre has been a keynote speaker at all five International Conferences, three of which Hoffman attended. Yet Hoffman does not

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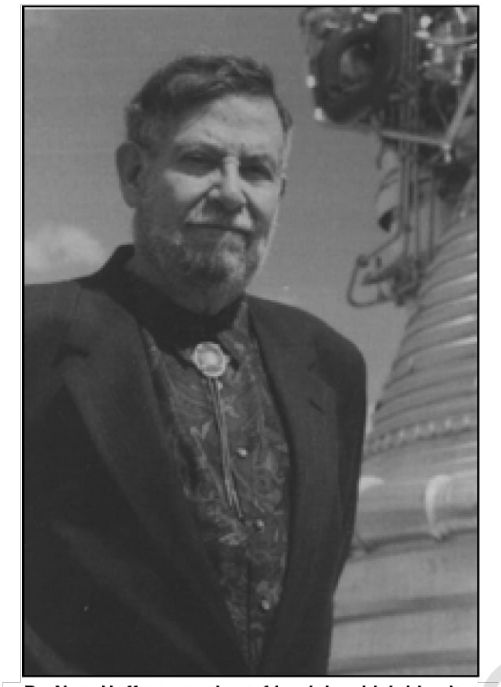
even mention McKubre's work. This is not a minor oversight; it is a fatal flaw. Hoffman makes many incorrect statements about calorimetry and experimental technique. If he had taken the time to look carefully at McKubre, or Pons and Fleischmann, Storms, or any of the other mainstream workers, he would have caught these mistakes. The experiments do not fit the patterns he describes. The potential errors he speculates about *could not have occurred*. The equipment and protocols are designed to rule out these errors. In many experiments, the magnitude of the excess heat alone rules out the errors he speculates about. For example, he mentions that bubbles on cooling coils can sometimes insulate the electrolyte, raising the temperature slightly. In leading experiments the temperature rose thousands of times higher than bubble formation could explain.

For people who already know the field, this book is old hat, or a stroll down memory lane. A scientist who is thinking about doing these experiments for the first time who knows nothing about the field will find valuable information, although I recommend reading the original sources instead. Hoffman's writing style is clear and forceful. His own comments are often helpful, and to his credit he has brought good material from wide-ranging sources, like the checklist of potential errors measuring tritium, from Carol Talcott-Storms on page 61, the statistical analysis by Tolly on page 89, and the Appendices written by G. Miley and B. Oliver. The bibliography is pretty good too, except, of course, it does not list the major papers about excess heat. (One or two minor papers about heat did creep into the chapter about tritium).

**A Cute Presentation Format, But It Gets on Your Nerves**

This book is written in an odd but appealing format. It is an imaginary Socratic conversation; a series of questions and answers between a Young Scientist (**YS**) and an Old Metallurgist (**OM**, Hoffman's alter ego). I have never seen a serious scientific book written as an imaginary conversation. In the chapters on history and the overview this format is effective, but in the technical chapters it becomes tiresome.

The tone of the writing is irritating. It is hypocritical. Hoffman trashes cold fusion by pretending the dramatic successful experiments never happened, and by grossly misrepresenting other experiments. He pretends to be impartial. He even makes a show of being solicitous toward the cold fusion scientists. He uses little of the heavy-handed mockery and harsh language you see in attacks against cold fusion by Huizenga, Taubes or Morrison. Unlike the American Physical Society officials, he makes no accusations of criminality or lunacy, he does not say that cold fusion scientists are half-wit suicidal Branch Davidians(6). Hoffman criticizes Taubes. Occasionally, he praises a few experiments. In a short chapter titled "Experiments Without Apparent Artifacts,"



**Dr. Nate Hoffman, author of book in which his alter ego (by his assertion) is the "OM"—Old Metallurgist**

Hoffman says that Chien's experiment at Texas A&M, Claytor's work at Los Alamos, and a few other experiments seem to be free of error. He did not include any that showed excess heat. (He says that Russ George's work at E-Quest is "interesting," but he does not mention that it produces excess heat.) His Mister Nice Guy tone and his occasionally pat-on-the-head praise of a few cold fusion scientists will make a naïve reader think that Hoffman really is even-handed, and he really has presented "a broad review" conducted in an "open minded" fashion, as the book jacket blurbs claim. A blurb from Steve Jones says:

> "Dr. Hoffman's treatise on anomalous nuclear effects in deuterided materials has been looked upon as pro-cold-fusion and paradoxically as anti-cold-fusion. It is neither. Rather, I see Dr. Hoffman's book as a significant effort to scrutinize relevant observations with care . . ."

This book does not "scrutinize relevant observations." It buries them! It ignores them, just as Jones himself does. This is a narrow, biased, anti-cold-fusion tract masquerading as a broad review. The relevant data has been stripped out because it contradicts the point of view Hoffman, Schneider, and Jones want to foist on the reader. This book demonstrates why you should never depend upon a review of a scientific field, particularly a controversial field. You must also read original sources.

**Masterful Analysis of Non-Issues**

Many of the topics Hoffman chose to address are non-existent, non-issues. His analysis and presentation of these topics is sometimes masterful. He does a superb job in describing the problems, he includes the details, facts, and hard numbers you need when you check the literature to see if these problems marred the experiments. But he does not say that the hypothetical problems he describes do not exist in the real world.

The short chapter about calorimetry "Possible Artifacts Associated with Heat Measurements". . . and its whimsical phantom bibliography is described above. It is a masterpiece of obfuscation. In it, Hoffman carefully avoids saying anything about any actual cold fusion experimental result. On the surface, it is a technical discussion of hard facts. He talks about the Joule-Thompson effect in detail. Taking a 10.6 gram cathode (which he says is a "typical mass," but which I think is larger than usual), he says: "The maximum loss of deuterium is from a stoichiometry corresponding to pressures of 0.2 in the shear modulus, which occurs at a loading of about $PdD_{0.9}$, to the stoichiometry of one atmosphere of $D_2$, which occurs at roughly $PdD_{0.65}$ as one can see from this phase diagram (Figure 6.2). " Working through the diagrams and figures, we find that with 10.6 grams of palladium, under extremely rare circumstances, the Joule-Thompson effect might slowly store and then suddenly release a one-time burst as large as 2.18 kilojoules. With some types of calorimeters, under some circumstances, this might appear to be a positive burst, rather than zero-sum energy storage and release. In many cold fusion experiments, including the ones at SRI, continuous generation from small cathodes adds up to far more than 2.18 kilojoules. Some cathodes have produced a hundred thousand times more energy than this. Therefore, cold fusion cannot be an artifact of the Joule-Thompson effect. Hoffman leaves the reader with the impression that this might be a significant artifact even though his own analysis proves it is not.

Other topics discussed briefly include the effect bubble formation on the insulating quality of the cell and mixing. In any calorimeter, it is essential that you ensure the cooling fluid or electrolyte is properly mixed. Everyone doing cold fusion knows this, and in every experiment I have read about, proper mixing is assured. Mixing is important but so elementary that no scientist would overlook it.

Sometimes he goes to extremes, raising points that he must know are disingenuous. The worst example is at the end of the chapter where the gullible Young Scientist asks: "do the experimenters use rms voltmeters?" The Old Metallurgist answers like a crafty lawyer:

> "**OM:** To my knowledge, no electrochemist involved in these calorimetric studies uses rms meters. Calorimetry specialists have raised this point many times, but the heat-measuring cold fusion community has rather 'heatedly' replied that rms meters are not required.
> "**YS:** On what basis do they justify not using rms meters?
> "**OM:** Their claim is that they routinely examine the output signal with cathode-ray tubes and see no signal that is hashy enough to justify rms meters.
> "**YS:** Certainly a cathode-ray tube readout should be a sufficient instrument for ruling out the necessity of rms voltmeters, but I'm surprised. . . "

First of all, this is nonsense. Many cold fusion scientists use rms meters to mea-

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sure power, as well as oscilloscopes, but more to the point, they all use computer systems with fast data acquisition boards. These not only measure true rms power better than most meters, but they also integrate power to measure energy. Nobody relies upon a meter. Does Hoffman imagine that graduate students are posted to watch a meter 24 hours a day, writing down the power levels and graphing them by hand?

The chapter concludes:

"**OM:** Well, that is a quick rundown on possible heat artifacts. In general, these heat measurements are being done by very knowledgeable experimenters who know how to avoid artifacts."

How kind of Hoffman to mention this! As Anthony put it: "For Brutus is an honorable man; So are they all, all honorable men. . . "

**Mind Boggling Mistakes**

Every so often, Hoffman goes off on a tangent and makes wildly incorrect statements. Most of chapter 3, Radioactivity Artifacts, is devoted to a discussion of used moderator water from fission reactors, which he believes may be causing artifacts in some cold fusion experiments. The discussion begins:

"**OM:** There are strong indications that commercially sold heavy water may contain variable contents of used moderator water from either CANDU-type nuclear reactors or Savannah River-type weapons production reactors.
"**YS:** What would indicate that?
"**OM:** The indicator is the enormous variation in the tritium-to-deuterium (T/D) ratio in different batches of heavy water. . .
"**YS:** But why in the world would the commercial suppliers mix any used moderator water into their feedstocks?
"**OM:** As to why, one cannot just pour used moderator heavy water from these reactors down the drain- the tritium content is too high- and besides, heavy water is not all that cheap . . ."

This is followed by a five-page, in-depth discussion of how used moderator water might affect a cold fusion experiment. Hoffman speculates that uranium from the reactor might contaminate the water. He includes a table of Alpha Emissions for a nanogram of uranium decay isotopes. The first three pages of the next chapter describe how corrosion products from zirconium fission fuel cladding in commercial heavy water probably explain the isotopic and chemical changes seen in palladium cathode surfaces. This is a carefully presented, detailed, professional-looking exposition. It is also a preposterous flight of fancy. The levels of tritium and other dangerous contaminants in used moderator water are millions of times too high for the water to be cleaned up and sold to the public for any purpose. I faxed this section to Ontario Hydro, the company that produces 90% of the world's supply of heavy water for use in their Canadian designed CANDU reactors. They responded with a detailed two-page fax, in which they show that Hoffman's idea is, quote: *pure nonsense*. They said that the specific activity of tritium in newly refined virgin heavy water varies from 0.05 to 0.17 μCi/kg (microcuries/kilogram), depending on the method of refining and the site of the refinery plant. Tritium levels above 2.0 μCi/kg is considered dangerously radioactive, and must be handled by trained personnel wearing appropriate protective clothing, using special equipment. In an operational CANDU power reactor, the moderator water contains $10^7$ μCi/kg, and research reactors generally contain $10^6$ μCi/kg. Furthermore, this water contains many other contaminants even more dangerous than tritium, including long-lived alpha and gamma emitters. Removing these radioactive contaminants would be far more expensive than refining virgin heavy water from ordinary water. In order to reduce the tritium to safe levels you would have to mix the moderator water with virgin heavy water in a ratio of 1 to 100,000,000. Ontario Hydro concludes:

"Used moderator water can often be re-sold, but only to other reactor operators. . . . Ontario Hydro dominates the world's nuclear market for heavy water and the world's non-nuclear wholesale market, and we have never attempted to use diluted, cleaned-up old moderator water for our non-nuclear markets."

The letter points out that some of Hoffman's other speculation about heavy water is also wrong:

"Theoretically, deuterium gas produced by electrolysis has lower D/T ratio than the source $D_2O$. However, in practice, there is no difference, as the separation factor is so small, and the operation is done in a single stage process. . . . [Hoffman's] suggestion that commercial $D_2$ gas suppliers must start with heavy water that is higher in tritium activity than any commercially available product any is also pure nonsense. Ontario Hydro is one of the world's major deuterium gas suppliers and we provide heavy water to one of the world's other major suppliers. The heavy water starting material is our normal virgin product."

Hoffman went badly astray here. He made a wild guess that was wrong by eight orders of magnitude. He should have called Ontario Hydro to check out these weird ideas. Why didn't his editor or Schneider catch this? Did Schneider read the book before he wrote the Forward? The text that Hoffman himself wrote (excluding material from other authors) is only 130 pages, and he reportedly worked on it for several years. In all that time he should have taken a few minutes to call Ontario Hydro. This is not a minor issue. If Hoffman had tucked this idea in a footnote, as a throwaway suggestion, then I suppose his editors and reviewers might have overlooked it. But he devoted eight pages to it! Six percent of the book! He devotes zero pages to excess heat; power density; temperature; triggering mechanisms; current density; cathode testing techniques and other topics most people think are the key to the phenomenon. And that is not the only glaring error in the book. It is the most egregious mistake, but the statements about rms meters and calorimetry run a close second.

Hoffman's publisher and reviewers at the American Nuclear Society and EPRI are among the world's leading experts in nuclear fission. EPRI performs the most advanced nuclear power reactor research and development in the world. They spend hundreds of millions of dollars designing new fission power reactors. How could a leading expert at EPRI like Schneider fail to realize that no chemical supply house could sell used heavy water? Jones asserts that Hoffman is *careful*. Did Jones read the book? I believe that Hoffman, Schneider, Jones and the others did not overlook these mistakes. They ignored them, because they do not take this field seriously. They do not care whether the arguments marshaled against cold fusion are scientifically correct or *pure nonsense*. They will use any tactic to discredit the field. They ignore positive experiments; they pretend that McKubre does not exist; and they toss out scientifically illiterate arguments like *used moderator water*. They publish these absurd arguments on Internet and in books like this in order to confuse the issue. They want to drown out reasoned scientific debate with chaotic nonsense and mind-boggling mistakes. They want to lower the standards of science so that instead of looking at carefully derived data, superb calorimetry, and meticulously worked out theories, we go chasing off after used moderator water and other phantasmagoria.

**Focus on Botched Experiments**

Hoffman devotes much of this book to statements from people who are not qualified to discuss cold fusion. This reaches extremes in Chapter 8, in which he describes a farcical scientific conference held in 1991 called the Half Moon Bay Review, which was billed as an "in-depth review of cold fusion." It was sponsored by Schneider and arranged by Hoffman himself. There were three ground rules:

"1. No firm 'believers' in cold fusion were to be included.
"2. 'Skeptics' were welcome as long as we felt that such a scientist could be objective about presented data.
"3. Each scientist panel member should be among the top few in the technical area for which he or she was selected."

In actual practice, Rule 1 meant that no competent cold fusion scientist was invited to join the Review Panel, and Rule 3 was ignored. The assembled "experts" specialize in neutrons and high-temperature plasma physics. Not a single one of them has <u>ever</u> published a serious paper on cold fusion or performed a cold fusion experiment. EPRI did not even invite their own expert electrochemist, McKubre, who is widely acknowledged as being one of the best in the world. This is like holding a conference on Tokamak hot fusion reactors and inviting only electrochemists.

The conference called in four experimental groups for the Review Panel to pass judgment upon: the Texas A&M cyclotron group (Kevin Wolf); Rocketdyne Helium Mass Spectroscopy Group; the Brigham Young University (BYU) 'Anomalous Nuclear Effects' Group; the Colorado School of Mines. Unfortunately, in 1991, none of these groups had ever done cold fusion calorimetry, which is at the heart of the

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experiment. The first two groups had assisted others in post-experiment analysis, but they themselves had not done any experiments. The Review Panel might have called in any number of scientists who had done competent experiments and seen positive results, but they decided not to. Their conclusion was pre-ordained, given the people they called in: "We were not presented with (nor are we aware of) any consistently reproducible data that shows anomalous nuclear effects in deuterium/metal systems. " Perhaps if they had invited a scientist who claimed to see such effects, he might have told them about it.

Since there was no one on the panel qualified to judge cold fusion, and since they only invited scientists whose experiments failed, I think this Conference was a deliberate attempt to discredit the field—just like the original DOE panel in 1989. If I am right, the review panel will never reconvene and call in the same groups again, because subsequently two of them saw definitive evidence of cold fusion. Kevin Wolf saw dramatic transmutations [See *IE*, Vol.1, No.2], which is exactly the sort of nuclear evidence the panel claimed it was looking for. Wolf himself denies that this is evidence of cold fusion; he claims that the transmutations have nothing to do with the fact that the cathodes were in a test tube undergoing electrolysis with heavy water. He thinks that by a fantastic coincidence, the cathode was hit by a hypothetical WIMP particle (or its products) from outer space! No other scientist with whom I have spoken believes that. Appendix D of this book is the report from the Rocketdyne Group, written in 1993. Long after the Half Moon Bay Review, Rocketdyne saw definitive evidence of helium in three groups of experiments. They dispute the first one, from China Lake (Miles), although their reasons are spurious. They do not dispute Texas A&M (Chien, Bockris) or E-Quest. Hoffman should have said this back in Chapter 8. He should have pointed out that subsequent work from two of these groups negated the conclusions of the panel.

Hoffman describes in loving detail some experiments performed by Steve Jones of BYU to detect low level neutrons. He devotes many pages to a whimsical experiment performed by Jones in which Portland cement was mixed with heavy water. This is interesting science. It sounds like fun. But it has nothing to with mainstream cold fusion. In any case, these experiments failed to produce definitive results. At ICCF4, Jones retracted most of his earlier claims. Hoffman writes a lot about this sort of marginal, flawed, retracted work, and he does not even mention hundreds of positive, replicated cold fusion experiments. In a short book on cold fusion, why would anyone devote several pages to Portland cement and never get around to mentioning McKubre or Kunimatsu? Hoffman also discusses Jones' palladium experiments, but these experiments prove nothing:

- The experiments were incompetently performed. No attention was paid to critical techniques like keeping the electrolyte and cathode clean. At Kamiokande, the workers were actually shown on Japanese NHK national television touching the metal surface of the cathode with their bare hands.(7) This is appalling sloppiness.
- They did not attempt to measure critical parameters like loading. They performed no pre-testing of cathodes. They made no attempt to raise current density to high enough levels, and they made no attempt trigger the reaction by heating up the sample or with some other well known method.
- They did not attempt to measure excess heat. There is no point to looking for neutrons unless you are sure you have a cold fusion reaction, and the only certain sign of a cold fusion reaction is a burst of excess heat. The heat is far easier to detect than the neutrons, and it is unambiguous. People have argued for years about the trace levels of neutrons detected in some cold fusion experiments. Jones himself has retracted some neutron measurements. In contrast, nobody has ever retracted any of the high power calorimetry (power above one watt), and the skeptics have never offered any serious arguments to disprove it. The level of neutrons in cold fusion is millions of times lower than it is for hot fusion, so looking for neutrons without first verifying the heat is folly.

Hoffman and Jones ignore one of the few things we know with certainty about cold fusion: it is nearly aneutronic. It does not produce neutrons commensurate with a hot fusion process. Cold fusion is nothing like fission or hot fusion. Suppose a scientist was given two samples of uranium and told to find out which one was undergoing intense fission. He could try measuring the surface temperature of the metal, but it would be far better to use a Geiger counter, because the principle signature of fission is ionizing radiation. The principle signature of cold fusion is heat. This fact was established during the first five minutes of the first cold fusion experiment that produced measurable levels of excess heat. It is known as *the dead graduate student problem*. If cold fusion was hot fusion, a cell producing a fraction of a watt of heat would generate such intense radiation that in a matter of minutes it would kill the people standing nearby. Yet many observers, including me, have stood next to cold fusion reactions and we are alive and well.

Jones and many others argue that all of those scientists who reported excess heat have made a mistake in their calorimetry. This is the heart of the cold fusion debate. Schneider says that in the Forward, in what I call *the subtle yet massive yet invisible unknown error hypothesis*. He says: "the alternative explanation, that the anomalous heat measurements are not from nuclear reactions but are the result of an unidentified error or artifact, appears to me to be the only viable explanation of 'cold fusion.'" He does not attempt to guess what error this might be. Schneider knows as well as I do that many cold fusion experiments have yielded very high power, some as high as 180 watts.(8) It takes a gigantic error to produce a false reading of 180 watts. Gigantic errors are inherently easy to spot. They are caught during calibration.(9)

Schneider wants to have it both ways. He wants an error so big that anyone would see it instantly, and so small that several thousand scientists have failed to see it over six years of intense research. The fact is, for the last 200 years, scientists have been using calorimeters to measure multiple-watt power levels. Measuring milliwatts is tricky, but watts are easy. There are few scientific techniques as cut and dry and as reliable as calorimetry in this domain. The basic techniques were described in my daughter's third-grade science textbook.(10) At any large high school science fair you will find experiments using calorimetry accurate and precise enough to measure the power levels observed in the leading cold fusion experiments. (I counted four calorimeters at last year's Dekalb County Fair.)

Schneider, Jones and Hoffman know there are no significant errors in the calorimetry of the mainstream professional scientists working in this field. They pretend there might be an error. They never specify which error, because they know there cannot be one. If the invisible error hypothesis really was "a viable explanation," as Schneider claims, he would list two or three potential candidates that fit the data. When pressed, opponents like Schneider and Hoffman always come up with a list of minor sources of error like the Joule-Thompson effect, which, thanks to Hoffman's own discussion, we see is many orders of magnitude too small to explain the data.

Schneider wants to artificially limit the debate to two options: either cold fusion is a standard hot-fusion style nuclear process, or it is a mistake. He wants to discuss these two hypotheses alone, excluding all others, even though these two were disproved years ago. He does not admit that cold fusion may be something new and different. He ignores the theoretical work of people like Schwinger, Hagelstein, Chubb and Chubb, and Preparata. He wants to limit the debate to two equally impossible, equally sterile hypotheses. Hoffman tries to steer clear of this issue, but he also pretends that Schwinger, Hagelstein and the others do not exist. Occasionally he hints that "a new paradigm " might be needed, but he does not mention any of the new theories. He does not give the name of a single theoretician!

**The Rockwell Rocketdyne Report**

Appendix D is a 35 page report written by Brian Oliver of Rockwell International, titled "Summary of Helium Analysis Conducted by Rocketdyne Under EPRI Funding." This is one of bright spots in the book. Since July 1989, Rockwell has worked with many cold fusion research labs around the world, looking for evidence of helium in used cathodes and in samples of effluent gas. They tested materials from the University of Utah, ETEC, China Lake, Osaka University, E-Quest, and others. Their procedures and instruments are described in detail.

The Rockwell Rocketdyne facility is wide-

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ly considered to be the best in the world at measuring helium. The hot fusion Tokamak programs contract with it to do helium analysis. Even John Huizenga, the arch-enemy of cold fusion, once said that if Rockwell found helium from a cold fusion experiment, he would endorse those results in writing. At ICCF4 Huizenga said that to Russ George and me. Unfortunately, his resolve did not last long. George and I talked to Hoffman and another Rockwell representative at the conference. We agreed we would find the money to pay for the work, and they agreed to test the George's E-Quest samples. When we told Huizenga, he panicked and reneged, saying he would not endorse the results after all. His exact words were: "I will leave it up to you fellows." Nevertheless, during that hour when he thought there was no chance Rockwell would agree, and no chance we would find a way to pay, Huizenga was confident enough to stake his reputation on Rockwell's work. It is a pity he chickened out because, as this chapter shows, Rockwell did test the E-Quest samples from the Los Alamos experiments, and their tests did prove that the excess heat comes from a nuclear reaction (at least in part). Experiments that did not generate excess heat showed 0.4 ppm helium. Experiments that did generate excess heat yielded helium far above that background level, at levels as high as 552 ppm, 100 times atmospheric concentration. Rockwell also looked at the ratio of $^3$He to $^4$He as well as $^{22}$Ne to $^4$He in the samples and found the isotopic ratios prove the helium could not possibly have come from contamination from normal terrestrial helium. This report says that these results have also been confirmed by other helium mass spectrometry at the U.S. Bureau of Mines (Amarillo, TX) and by SRI International. It says reactions in the E-Quest system are "characterized by the production of heat" but it glosses over important details. does not say that the power level is at hundreds of watts, or that the levels of $^3$He and $^4$He are proportional to the net energy produced by the device.

Hoffman mentions E-Quest briefly elsewhere in the book but he gives no details, except in one spot. In Table 10.1, on page 130, "Anomalous Nuclear Measurement Examples," he slips in a cryptic reference to E-Quest. He tries hard to disguise it. In this table, the name *E-Quest* is not mentioned; there is nothing about $^4$He; there is no reference to Appendix D; and naturally there is no mention of heat (a forbidden subject!). But he does include this comment which I suppose must refer to E-Quest:

"$^3$He; only one questionable measurement of $^3$He; one gas sample claimed to be from cavitation experiment showed high $^3$He."

It is hard to know what to make of this. Perhaps Hoffman finds his own laboratory at Rockwell "questionable" for reasons he does not reveal. Nobody else finds it questionable, but perhaps he has some inside information that calls into doubt Rockwell's ability to measure $^3$He at levels thousands of times above the natural background. From the phrase "claimed to be," I conclude that Hoffman does not fully believe the samples came from E-Quest's experiments at Los Alamos. I cannot imagine where else he thinks they might have come from. Perhaps he imagines that someone else at Los Alamos synthesized or obtained $^3$He, put it in $500 steel collection flasks, and shipped it to Rockwell as a joke.

Oliver's report shows that other tests, on cathodes and gas from other laboratories, were not as definitive as E-Quest, but some of them did show that helium has been created. Even when experiments yielded excess heat, most of the cathodes did not have elevated levels of helium or unnatural isotopic ratios. I suppose that means the reaction usually occurs near the surface, and the helium ash is lost with the effluent gas. Rockwell found clear positive results from two other groups besides E-Quest: China Lake, and Texas A&M.

Melvin Miles at China Lake (11) found helium in effluent gas samples proportional to excess heat. I discussed the Rockwell report with Miles. He pointed out that it is marred by misunderstandings and out-of-date information. Rockwell suspects that the helium may have come from contamination; that is, helium leaking through the glass. The report discusses this at length, but this possibility was ruled out years ago:

- The helium is proportional to the excess heat. There is no mechanism that would allow selective leaking in of helium through the glass weeks after a fraction of a watt of excess heat was generated. When you look a simple presence or absence test, the proof is even more stark. When there was no heat, there was no significant helium; when there was heat, there was easily detected levels of helium. The glass cannot know whether there was heat, and the excess was too small to weaken the glass or chemically change it to leak faster.
- Miles later repeated the experiments with steel collection flasks, and found the same results. Steel is more resistant to helium penetration than glass.
- Miles sent samples in both glass and steel collection flasks to other laboratories, including the University of Texas and SRI. They too confirmed the helium.
- The experiments are continuing, and Miles still observes excess heat and a positive correlation with helium. Over the past year, he has seen as much heat as his best earlier results. He has not yet been able to publish results from the last year.

Rockwell found no helium in the China Lake cathodes. Miles points out that he would not expect to find any, even if it had evolved inside the cathode during the excess heat runs. After every run with heavy water, the cathodes were electrolyzed with ordinary water to recalibrate.(12) This would probably purge any significant helium from the cathode. Miles told that to Rockwell, but they neglected to mention it in the report.

Oliver describes the analysis of the Chien (13,14) cathodes from Texas A&M in detail. After hemming and hawing, he concludes in italics:

"*Small amounts of $^4$He were observed in 7 of the 16 specimens analyzed*, 5 of these 7 were for specimens that had been etched. Six of the 7 'positive' $^4$He results were for AM-Pd4. Unlike the results obtained earlier, the results for the present group are more heterogeneous, and also do not show any clear correlation with either sample surface area (Figure D.6) or with etch depth (Figure D.7). *This would suggest that the small amounts of $^4$He observed in the Chun-Ching Chien cathode material from the electrolysis experiments performed in Professor Bockris's laboratory are indeed heterogeneous and extend to depths greater than ~4μm.*"

**Hoffman Argues With Himself**

The saddest part of this book comes close to the end, where Hoffman confronts the most important aspect of cold fusion: the heat. Heat is the principle signature of the reaction, and it is also the best, most iron-clad proof that the effect is real. I found this section heartbreaking. Here is the spectacle of a man who knows how to do good science; a man who knows the facts and the literature, who cannot bring himself to face the truth. Hoffman is stuck in a time warp, arguing about issues that were settled years ago, and issues that make no scientific sense. In the Socratic dialog format Hoffman appears to enjoy playing the devil's advocate and arguing with himself. It is playful. The Young Scientist acts as a foil, allowing the Old Metallurgist to strut his stuff and impress the reader, which he does at times. In this section, the argument gets out of hand. It gets ugly. A split personality emerges, as if he really is arguing with himself. I sense that Hoffman sincerely wants to confront the issue, but he cannot bring himself to do so. He skirts the issue. He approaches the answer, he gets to heart of the matter for a moment, and then suddenly he changes the subject, as if he cannot stand to look at the truth for long. The dialog begins:

"**YS:** I guess the real question has to be this: Is the heat real?
"**OM:** The simple facts are as follows. Scientists experienced in the area of calorimetric measurements are performing these experiments. Long periods occur with no heat production, then, occasionally, periods suddenly occur with apparent heat production. These scientists become irate when so-called experts call them charlatans. The occasions when apparent heat appears seem to be highly sensitive to the surface conditions of the palladium and are not reproducible at will."

Instead of answering the question, Hoffman has flown off on a tangent. The correct answer is: *Yes, because the techniques are proven and the signal to noise ratio is very high*. Those are the simple facts. Hoffman ignores them as he launches into an irrelevant and factually incorrect discussion. It is true that with some experiments heat is sporadic and unpredictable, but the simple answer remains: Yes because calorimeters measure sporadic heat reliably. Furthermore, Hoffman fails to say that Pons and Fleischmann, Patterson, Arata and many others have good control over the reaction. They can trigger it at will, and it

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continues indefinitely at steady, high power levels with such a large percent excess that there is no question it is real. Calling it "apparent heat production " is like watching a Boeing 747 take off and then saying "apparently it is flying." The conversation goes on:

"**YS:** Any phenomenon that is not reproducible at will is most likely not real.
"**OM:** People in the San Fernando valley, Japanese, Colombians, et al., will be glad to hear that earthquakes are not real.
"**YS:** Ouch. I deserved that. My comment was stupid.
"**OM:** A large number of people who should know better have parroted that inane statement. There are, however, many artifacts that can indicate a false period of heat production, as we have discussed. The question of whether heat is being produced is still open, though any such heat is not from deuterium atoms fusing with deuterium atoms to produce equal amounts of $^3$He + neutron and triton + proton. If the heat is real it must be from a different nuclear reaction or some totally unknown non-nuclear source of reactions with energies far above the electron-volt levels of chemical reactions."

This is the first and only place in the entire book where Hoffman faces the truth. But not for long. The question of whether heat is being produced is not open. It was closed, once and for all, back in 1991 when McKubre and others published their replications of Pons and Fleischmann. Hoffman finally makes this key point: the energies are far about the electron-volt levels of chemical reactions. It is hundreds of thousands of times above those levels, in some cases. Hoffman's statements about "many artifacts" and the heat "still being open to question" are false. Calorimetry at the multiple-watt level is cut-and-dry. It gives an unambiguous answer that no modern scientist can doubt or argue with. It is not complicated; the results are not open to question; the doubts and questions paraded by Hoffman and other skeptics have no scientific merit. The techniques used to measure watt-level heat are so fundamental, so easy, and the results so clear, that for Hoffman and others to question them is tantamount to questioning all of science back to James Watt. They might as well doubt that Newton's prism experiment proves white light contains the other visible colors.

Other skeptics have attempted to disprove the heat results. Hoffman does not. He does not even dare to mention the actual numbers from the papers. He is a good enough scientist to realize that he cannot challenge them. Instead, he pretends they do not exist. After coming so close to confronting the truth, Hoffman's imaginary dialog fades away in a meandering discussion of marginal experiments. This is typical of the whole sad book:

"**YS:** You were supposed to summarize 'cold fusion' for me. Your summary has no punch ending! You agree it is not the normal D-D fusion that was originally postulated. Does any anomalous nuclear effect exist or not? I suspect it does not. You apparently think that the answer is not in yet. Can you at least point to those experiments that you think should be continued?"

No scientist, anywhere, has ever postulated that cold fusion is *normal D-D fusion*. If it were, I would be dead from radiation. That is an infuriating straw man argument.

"**OM:** It's not an exclusive list -- and I'm talking about anomalous nuclear effects now, not heat. Obviously, the concrete setting experiments are interesting. The Los Alamos alternating disks of palladium and silicon with submicrosecond bursts of kilovolt charge are most intriguing. The cavitation experiments, which involve the creation of enormous pressures as bubbles collapse, seem worthwhile at this time."
"**YS:** Well, if you can't be more definitive on the reality of anomalous nuclear effects in deuterium/solid systems, at least you know what needs to be pursued.
"**OM:** Is there anything else you'd like to know about the anomalous nuclear effects in deuterium/solid systems? . . "

So . . . the heat drops out of the discussion, having lasted for only two paragraphs out of the entire book. Yet heat is the single most important aspect of cold fusion. It proves the three great essentials:

1. That cold fusion must exist, because the calorimetric signal to noise ratio of these experiments is so high and the techniques so reliable.
2. That it cannot be a chemical effect.
3. That it may well be the most important discovery in history, because pollution-free, high-density energy is the most vital resource on earth.

Hoffman sweeps the significance of the heat under the rug along with all heat experiments and data. Hoffman and the other skeptics want to talk about neutrons and Portland cement. They want to change the subject. They will never admit the heat is real, because that proves they have been drastically wrong from the beginning. Hoffman, Jones, and other hard-line opponents would dearly like to put a stop to all experiments that show excess heat. Hoffman wants to do Portland cement, he wants Los Alamos to continue looking for tritium. Cold fusion is fine as long as it is *not heat*. He wants to talk about E-Quest, but he dares not mention that they observe hundreds of watts of heat, and he deliberately leaves out their best helium results too, in Table 10.1, because he does not want to lend too much support to any aspect of cold fusion. The theme of the book, introduced in the Forward and repeated throughout is that all cold fusion results are marginal and probably mistakes. To prove that contention they censor out the dramatic successes like E-Quest's helium-4, even though Hoffman's own lab confirmed it, and even though it slipped into Appendix D. Above all, Hoffman and Schneider avoid giving any support to heat, no matter how dramatic and clear-cut the evidence for it is. They pretend that most of the top cold fusion papers do not exist. Yet heat is the key to cold fusion, and it is the key to mankind's long term survival.


**1.** M.C.H. McKubre, R. Rocha-Filho, S.I. Smedley, F.L. Tanzella, S. Crouch-Baker, T.O. Passell and J. Santucci, "Isothermal Flow Calorimetric Investigations of the D/Pd System," *The Science of Cold Fusion, Proceedings of the II Annual Conference on Cold Fusion*, Como, Italy, June 29-July 4, 1991, pp 419-443
**2.** M.C.H. McKubre, S. Crouch-Baker, A.M. Riley, R.C. Rocha-Filho, S.I. Smedley, and F.L. Tanzella (SRI International, Menlo Park, CA, USA), "Excess Power Production in $D_2O$ Electrolysis Cells: a Comparison of Results from Differing Cell Designs," presented at the Third International Conference on Cold Fusion, October 21-25, 1992
**3.** M.C.H. McKubre, B. Bush, S. Crouch-Baker, A. Hauser, N. Jevtic, T. Passell, S. Smedley, F. Tanzella, M. Williams, and S. Wing (SRI International, Menlo Park CA, Electric Power Research Inst., Palo Alto, CA), "Calorimetry Studies of the D/Pd System," presented at ICCF4
**4.** M. McKubre et al., "Isothermal flow calorimetric investigations of the D/Pd and H/Pd systems," *J. Electroanal. Chem.*, 368 (1994) 55.
**5.** M.C.H. McKubre (SRI International), Observations of Heat in Electrochemical Studies of the Palladium/Deuterium System Employing an Isothermal Flow Calorimeter.
**6.** F. Slakey (Science Policy Administrator, American Physical Society), "When the lights of reason go out," New Scientist, Sept. 1993.
7. F. Nakano, "The Reality of Cold Fusion Can No Longer Be Denied," *Bungei Shunju*, Sept. 1991.
**8.** M. Fleischmann (Univ. Southampton), S. Pons (IMRA Europe), "Calorimetry of the Pd-D2O system: from simplicity via complications to simplicity," *Physics Letters A*, 176 (1993) 118-129
**9.** Actually, an error of that magnitude would never happen in the first place. If the instruments were broken or installed incorrectly to that extent, they would show nothing. A modern calorimeter uses digital thermometers and computer data acquisition. With such instruments, a 180 watt error is as unlikely as an error that causes a digital clock to run two hours fast one day, and two hours slow the next. When an electronic clock or thermometer breaks that badly, it stops, goes blank, and shows nothing.
**10.** M. Cohen et al., Discover Science, (Scott, Foresmann), pp. 158 - 159. (Note: While the principles described in this textbook are correct, the experiment as described does not yield the results shown. The authors are so certain that calorimetry works, they did not bother to verify that the particular setup they illustrate will work. I tested it and found that it does not work.)
**11.** M. H. Miles, R. A. Hollins (Chem. Div., Research Dept., Naval Air Warfare Center, China Lake, California), B. F. Bush, J. J. Lagowski (Dept. Chem., Univ. of Texas), and R. R. Miles (Nuc. Safety Dept., EG&G, Colorado), "Correlation of Excess Power and Helium Production During D2O and H2O Electrolysis Using Palladium Cathodes," *J. Electroanalytical Chem.*, vol. 346, nos. 1-2, 1993, pp 99-117.
**12.** To recalibrate means to recheck the equipment after a run and make sure the temperature still rises the same extent in response to a given input power level.
**13.** Chun-ching Chien (Chem. Div., Inst. Nuc. Energy Res., Taiwan), Dalibor Hodko (on leave from Rudjer Boskovic Inst., Zagreb, Croatia), Zoran Minevski and John O'M. Bockris (Chem. Dept., Texas A&M Univ., Texas), "On an Electrode Producing Massive Quantities of Tritium and Helium," *J. Electroanalytical Chem.*, vol. 338, 1992, pp 189-212
**14.** Chun-ching Chien and Theresa Chen Huang (Inst. Nucl. Energy Res., Chem. Div., Lung-Tan, Taiwan), "Tritium Production by Electrolysis of Heavy Water," *Fusion Technology*, vol. 22, no. 3, 1992, pp 391-394


## Editor's Comment:
## *A Brief History of a Book*
**by Eugene Mallove**

Harsh as it may be, Jed Rothwell's negative appraisal of Dr. Nate Hoffman's cold fusion book is right on target. But there are other matters surrounding the publication of this book that readers should know about. I spoke with Nate Hoffman and EPRI's Dr. Tom Passell to obtain this background. Dr. Passell has been the man at EPRI who has actively supported cold fusion research—particularly the work at SRI, in contrast to Dr. Schneider at EPRI, who has long been its antagonist.

An egregious omission from the Hoffman book are the gamma ray spectra obtained in the laboratory of Dr. Kevin Wolf of Texas A&M University (See *IE* #2 May/June 1995). How could this be, when this Wolf work was funded by Schneider's division in EPRI? Both Wolf and Schneider have been trying (unsuccessfully) to keep a tight lid on these spectacular results, because neither of them are willing to admit that there is *any connection* between the excess heat in Pons-Fleischmann-type cells and the unambiguous transmutation of elements that Wolf obtained while conducting a Pons-Fleischmann type experiment. ***(Continued on page 59)***

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**Dr. Peter Graneau**
(Northeastern University Center for Electromagnetic Research)
"Anomalous Forces in Water Plasma Explosions"

**TAPE#2** →

**Buford ("Ray") Conley**
(MIT, graduate student, Dept. of Aero/Astro Eng.)
"Light Water Excess Heat Experiments"
**Fred Jaeger**
(ENECO, Salt Lake City)
"Patents and Commercialization"
**Professor Peter L. Hagelstein**
(MIT Dept. of Electrical Engineering and Computer Science)
"Cold Fusion: What We Know and What We Don't Know"

**TAPE#3**

**Professor Peter Hagelstein** →
(CONTINUED from TAPE #2)
"Cold Fusion: What We Know and What We Don't Know"
**Professor Keith Johnson**
(MIT Dept. of Materials Science and Engineering)
"Progress in the Theory of Excess Heat" *and*
*Excess Heat*- The Movie, now being made by Hollywood!
**James L. Griggs**
(Hydro Dynamics, Inc., Rome, Georgia, USA)
"The Hydrosonic Pump: Excess Power and Other Effects"

**TAPE#4**

**Bertil Werjefelt** →
(PolyTech(USA), Hawaii)
"Energy from Magnetic Materials and Magnetic Fields"
**Dr. Eugene F. Mallove**
(Cold Fusion Technology, Concord, NH)
Concluding Remarks

The tapes include the speakers' presentations as well as question and answer sessions. There is footage of the Hydrosonic Pump under test as well as close-up footage of rotor surface melting effects. Bertil Werjefelt shows footage of experiments in his own laboratory. There are scenes of Japanese researchers from Sumitomo, Hitachi, and other companies and universities now involved in their claimed extraction of energy from magnetic materials. These efforts are now said by these researchers to be proceeding toward commercialization. Actual experiments are seen in progress. The tapes are VHS cassettes in NTSC format and range from 120 to 127 minutes each.

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***(Continued from page 58)***

Dr. Hoffman told me that he started work on his book in 1989, when Dr. Schneider contracted with his Rockwell laboratory to "help anyone doing experiments look for nuclear ash." According to Hoffman, his group has received some $300,000—for experiments, organizing a few review meetings (such as Half Moon Bay and the NSF/EPRI *closed door* conference in the fall of 1989), travel, and writing his book. One might say that Hoffman's book is his "final report" to Schneider.

Hoffman maintains that he asked Kevin Wolf to write a chapter on the gamma ray spectra for his book. He says that Wolf initially agreed to do so, but then, after the ICCF4 conference in Maui (at which Wolf was to speak but was instead a mysterious no-show), Wolf said that he would not write a contribution to Hoffman's book. We find only a brief reference by Hoffman to Wolf's results. On page 130 we see the entry near Wolf's name: "data appear real" for gammas, unusual radioisotopes, and x-rays. Yet nowhere in the entire book are these critical data described! This is a travesty. One of the tenets of scientific research is to report all data of significance connected with any experiment—even if that data appear to contradict one's belief in what is possible.

Who is to blame for what can only be called a serious breach of scientific ethics? Is it Schneider? Is it Wolf? Is it Hoffman, who tacitly went along with this cover-up? After all, Hoffman could have refused to sign his name to such a book—right? He had *some* control over Schneider, because he told me that he insisted to Schneider that Schneider use the language "in my opinion" for the negative comments about cold fusion that he made in the book's introduction. Hoffman says that Schneider wanted the Wolf results published. Personally, I wouldn't believe such a statement from Schneider if it were written in blood.

Hoffman told me that he had most of his book written in draft form by some time in 1991. So why does the book appear in 1995, and without any description of Wolf's data—long after they were available? Moreover, why did this book appear without key data for which Schneider at EPRI provided the funding? This funding derives ultimately from American electric utility customers, since it is they who contribute to the research of EPRI. Are the American people entitled to know that their utility bills have paid for a possible golden path toward a sustainable energy future?

Apparently Schneider, Wolf, and Hoffman are unaware of the moral dimensions of what they are doing. They think that cold fusion is some kind of academic debating issue, when in truth it is the well-being of the planet that is at stake. Because Dr. Hoffman is a graduate of Technion in Israel. I asked him whether he had ever encouraged any of his contacts there to look into the cold fusion matter. He said that he had not. Hoffman *claims* to be "impartial" in this drama. Does that mean he thinks there is a 50% chance that cold fusion excess heat, potentially explainable by nuclear reactions, is real? If so, he would have written a far different book. I must conclude that Dr. Hoffman is either naïve, devious, or both.

In 1991 this reviewer concluded in *Fire from Ice* that the evidence for cold fusion —whatever its explanation turned out to be— was "overwhelmingly compelling." Why in 1995 does Hoffman, equipped with much more overwhelming evidence, *claim* to be still sitting on the fence? Once again, the *politics* of cold fusion have proved to be as bizarre as the very real phenomenon itself.

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**Eugene F. Mallove, Sc.D., Editor-in-Chief and Publisher**

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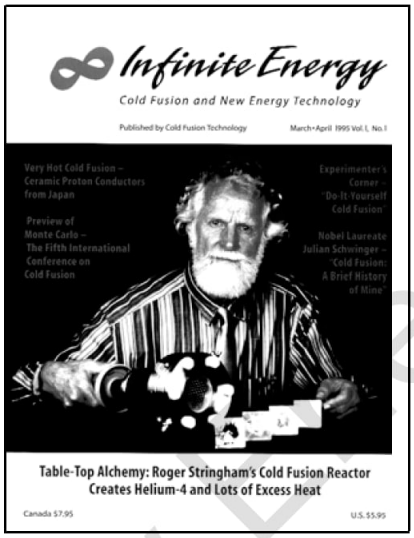


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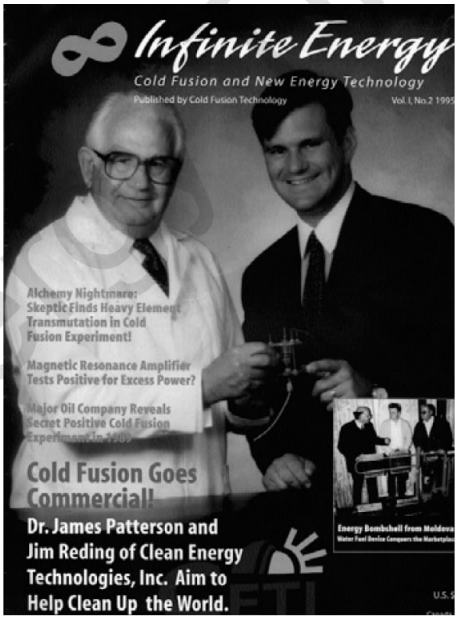


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